# THE MANKIND QUARTERLY

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# Man
# Between Past and Future

BRUNETTO CHIARELLI
*University of Florence, Italy*

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VOL. XXXVIII, Nos. 1 & 2 Fall/Winter 1997

## CONTENTS

Fatherhood as a Deterrent against Female Promiscuity: A Time to Refurbish the Electra Complex
By Nancy S. Coney and Wade Mackey ..... 3
A Humor Workshop Program to Aid Coping with Life Stress:
By Michael G. Lowis ..... 25
African Exodus: Re-Evaluating an Hypothesis
By Edward M. Miller ..... 39
The Decline of the *Eppur si muove* Spirit in North American Science: Professional Organizations and PC Pressures
By John J. Furedy ..... 55
Hans Jurgens Eysenck: Obituary
By Chris Brand ..... 67
John C. Flanagan and the Development of Psychological Surveys: The U.S. Aviation Psychology Program
By Roger Pearson ..... 85
Raymond B. Cattell and the Fourth Inquisition
By Glayde Whitney ..... 99
Raymond B. Cattell: A Lifetime of Achievement
By Kevin Lamb ..... 127
Book Reviews ..... 182

*Cover Picture:* Raymond B. Cattell, Distinguished Psychology Research Professor Emeritus. (See Articles: *Raymond B. Cattell and the Fourth Inquisition*; and *Raymond B. Cattell: A Lifetime of Achievement*.)

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# Fatherhood as a Deterrent against Female Promiscuity: A Time to Refurbish the Electra Complex

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It is argued that the presence of a father, during the daughter's formative years, acts as a deterrent against his daughter-grown-to-maturity becoming promiscuous. Given that multiple sexual partners is the best predictor for contracting a sexually transmitted disease, rates of sexually transmitted diseases in the U.S. were compared to rates of out-of-wedlock births across the U.S. The results were significant. As rates of out-of-wedlock births increased, rates of sexually transmitted diseases also increased. The association occurred (1) if rates of out-of-wedlock births and rates of sexually transmitted diseases were surveyed from the same time frame as well as (2) if the rates of out-of-wedlock births were surveyed from a *prior* generation and the rates of sexually transmitted diseases were surveyed from a *subsequent* generation. Neither pattern held for rates of divorce and rates of sexually transmitted diseases. It is further suggested that no extant theory on the female psyche or motivation hierarchy would predict that early father presence with his daughter would deter later promiscuity on her part, but that such a theory or model would be desirable.

KEY WORDS: Father-child relations, out-of-wedlock births, divorce, sexually transmitted diseases, psychology of women.

Rule #1: All politics are local. *Rep. Tip O'Neill*
Rule #2: All long term politics are reproduction strategies
Rule #3: Effective long term politics camouflage Rule #2. *Ipsoc Macquire*

*Homo sapiens* is a separate species and thus, by definition, is unique. In addition to their singularity by achieving the rank of species, humans possess an oddity which is rare in the biota and virtually idiosyncratic in large, terrestrial primates. Humans have social fathers. Across the world's community of cultures, men marry. Once married, the overwhelming majority of the husbands become



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fathers. Once fatherhood is achieved, the fathers willingly share their treasure – their time, their emotions, and their food and resources – with their young (Hewlett 1992, HRAF 1949, Lamb 1987, Mackey 1985, 1996). Any rare exception to this trend receives much publicity and analysis. For example, when the Ik of Kenya were experiencing severe famine and deprivation, children were nurtured only until the age of three and were then expected to fend for themselves (Turnbull 1972). Turnbull's ethnography of the Ik regularly focused on the lack of normative parenting (from mothers and fathers).

The adult males of the (large) terrestrial primates, viz. chimpanzees, gorillas, baboons, essentially leave parenting to the mothers. The adult males will certainly challenge predators that would threaten the troop's young. Nonetheless the day-to-day care of the troop's young is left to the mothers (Fossey 1983, Goodall 1986, 1988, Jolly 1985, Schaller 1964, Smuts 1985, Smuts et al. 1986, cf Hrdy 1977, Hausfater & Hrdy 1984). As a matter of contrast, canid adult males are highly solicitous of their young and – not unlike men – uniquely provision as well as protect their young, as with wolves (Mech 1966, 1970; Mowat 1963, Murie, 1944), coyotes (Dobie 1949, McMahan 1976, Ryden 1974, Young & Jackson 1951), jackals (Lawick & Lawick-Goodall 1971, Moehiman 1980), hunting dogs (Kuhme 1965), and foxes (Alderton 1994). That is, adult male canids (1) leave the perimeter of their group, (2) procure food, (3) return the food to the group, and (4) feed their young (and their mates). No adult male primate, other than men, does this. Virtually all adult male canids do. Convergent evolution seems like an appropriate concept to apply to this particular consonance between men and adult male canids.

Because neither adult male chimpanzees nor adult male gorillas systematically provision their young, but men do, the tendency probably occurred after the Hominid/Pongid split. Thus, there are several million years of evolution available during which the psychologies of fathers and the psychologies of daughters have had the opportunity to adapt to each other. This exercise explores one such putative adaptation.

## Parenting

Intensive "mothering" is as old as mammals themselves. Intensive



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"fathering", as seen above, is more erratic and for large terrestrial primates non-existent. The question can be asked: "What psychological adjustments may have occurred between the father and the daughter as the daughter grew to maturity under the aegis and the provisioning of the father?" One dynamic seems clear: the father-daughter relationship was not and is not sexual. Although father-daughter incest does occur, and does seem to occur more than mother-son incest, father-daughter incest is rare (de Young 1987, Kluft 1990, Shepher 1983 Kinsey, Pomeroy and Martin 1953, Finkelhor 1979, Finkelhor and Dziuba-Leatherman 1994). The young daughter is much more at risk of sexual abuse if her social and biological father is not proximate. Adjunct boyfriends of the mother and step-fathers are several orders of magnitude more likely to abuse the immature girl than is the social and biological father (Gordon and Creighton 1988, Russell 1986, Tyler 1986; see Immerman and Mackey [1997] for *sequelae* to sexual abuse of a pre-pubescent girl). Such a father acts as an insurance policy against his daughter's sexual exploitation. It is argued below that an affiliative bond is constructed between the father and his daughter and that one of the consequences of that affiliative bond is a more coherent mating template of the young-daughter-grown-to-maturity.

**Prolegomenon**

Three assumptions/premises are germane to this argument. First, it is accepted here that women have not adopted a chimpanzee mating strategy of multiple sexual partners per breeding season (Goodall 1986,1988, Waal, 1982, 1984, 1990, Turke 1984). Whatever the mix between nature and nurture, the end result is that cultures effectively discourage married women from having multiple partners (adultery) (Broude 1980, Schlegel 1972, Van den Berghe 1979, Stephens 1963, Divale and Harris 1976, Fisher 1972, 1992, cf Kurland 1979 and Immerman and Mackey in press). To the extent that the *Sex in America* survey is valid and generalizable, for any given year the large majority of married women in the U.S. mate only with their husbands (Michaels, et al., 1994. See Kost and Forrest, 1992, for a similar conclusion). While the incidence of cuckoldry *cum* issue, which is unknown to husbands, is not zero, it appears to be close to error variance (1 % - 3% of births). (Ellis and Walsh 1997, Brock and



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Schrimpton 1991). Thus, when a girl is born, she is normally born into and raised within a biocultural mosaic wherein her mother has a very restricted range of mating partners. For any given year – rather than for a life-time or ever – the mean, median, and mode of the mother's sexual partners is probably close to one. Accordingly, the daughter's maternal role model would reflect a tendency to have restricted numbers of mating partners.

Second, it is argued that for most females the first and most intensive interaction with an adult male is with her father.[1] Because the father is, at base, a constant, she would have a consistent, perhaps a constant, image of a trusted and loved adult male figure. A lack of a father would seem to create a more amorphous and nebulous imagery of "adult masculinity". Accordingly, a constant image of a father, i.e. of a man, would more crystallize for the daughter-grown-to-adulthood an "adult male masculine image". Conversely, a lack of a constant image of a man – i.e. the absence of a father – would create a more diffuse image or template for evaluating the mating partners for the daughter-grown-to-adulthood. Framed differently, the notion of "imprinting" seems appropriate. A generalized definition of "Imprinting" would include the organization of a system which is time-specific, permanent, and relatively resistant to extinction (see Freedman and Gorman 1993, Hess 1973, Lorenz 1935, Tinbergen 1951, 1965, cf Rossi and Rossi 1990 and Perusse et al. 1994; see Bornstein [1989] and Leland [1994] for overviews). In this instance, the young girl's experiences with her father would be expected to have lasting effects upon her development and organization of an effective psycho-sexual template: her reproductive tactics.

Third, several countries, e.g. the U.S., Sweden, Denmark, (i) have created a *de facto* social experiment in which a set of expectations has been generated that treat social fathers as either optional or supernumerary and (ii) tend to collect fairly clean demographic data. (In 1993, 47% of all live births in Denmark were

[1] Although not developed in this article an analogous value or function of the resident social and biological father vis-a-vis his son could be argued to be that the father lowers the chances of violent behavior on the part of his son-grown-to-adulthood.



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to unmarried mothers. The analogous figures in Sweden and the U.S. were 50% and 31% respectively (U.S. Bureau of the Census 1996). Divorces, of course, would add to the number of fatherless minor children to the extent that mothers [rather than fathers] received custody). The U.S. collects demographic data by state plus the District of Columbia. Hence, if data are analyzed across states (plus D.C.), an "n" of 51 becomes potential.

**Hypothesis**

Using the above three constructs, the hypothesis can be generated that girls raised without fathers (i.e. born-out-of-wedlock or involved in a divorce) would have a less-well-developed or coherent or organized mating-template when compared to girls raised with their biological and social fathers. Thus, it would be expected that girls who are raised without a father would be less discerning, i.e. have more sexual partners, than girls who are raised with a co-residing father.[2]

## METHOD

*The dependent variable*

In the U.S., as elsewhere, sexual activity is a secreted activity. Accordingly, compiling data via naturalistic observation would be difficult, highly illegal, and unwelcomed by all. Thus, a proxy for "multiple sexual partners" becomes necessary. It has been noted by several authors (Aral, Mosher, and Cates 1990, Brunham and Plummer 1990, Hunter et al. 1994, Laumann et al. 1997, Moore and Cates 1990, Westrom and Mardh 1990) that multiple sexual partners is the best marker for acquiring a sexually transmitted disease: an STD. Because the Division for STD Prevention compiles data by state (plus D.C.), rates of STDs were used as markers for multiple sexual partners. The two STDs to be analyzed were rates of gonorrhea (in females) per 100,000 population and congenital syphilis per 100,000 live births.

It should be noted that if an individual contracts an STD, the infection does not necessitate that the individual has had multiple

[2] A possible exception would be the avunculate (Schlegel 1972).



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sexual partners. However, if someone is infected with an STD, then that someone either has had multiple partners or has chosen a mating partner who has had multiple partners (see Robinson 1950 for an early discussion of the problem, and see Borgatta and Jackson 1980, Goodman 1959, Hanushek, Jackson and Kain 1974, King 1997, Langbein and Lichtman 1978, Pedhazur 1982 for subsequent discussions plus partial solutions to the problem of relating aggregate data to individual behavior). Suffice it to say that the analysis below is not construed to specify how any one individual would or would not behave. The analysis below is content to attempt to discover what – if any – behavior patterns are aligned with other behavior patterns.

## RESULTS

### Out-of-wedlockbirths and gonorrhea:1991-1993

For all three years, 1991-1993, the percentage of all births which were out-of-wedlock births and rates of gonorrhea were significantly and positively related. See Table 1. As would be expected, the mean correlation for the three year interval was also significant and positive ($r_p$ =.804, p < .001, df = 49). Accordingly, nearly two-thirds ($.804^2$ = .646 = 64.6%) of the variance in gonorrhea can be explained by knowledge of the level of out-of-wedlock births.

### Out-of-wedlockbirths and congenital syphilis:1991-1993

For all three years 1991-1993, the percentage of all births which were out-of-wedlock births and rates of congenital syphilis were significantly and positively related. See Table 1. The mean correlation for the three year interval was significant and positive ($r_p$ = .752, p < .001; df = 49). Over half ($.752^2$ = .566 = 56.6%) of the variance in congenital syphilis could be attributed to changes in percentages of out-of-wedlock births. Thus, for father preclusion – as indexed by out-of-wedlock births – the hypothesis is substantiatial.

### Divorce and STDs (gonorrhea and congenital syphilis) 1991-1993.

For all six combinations of years and STDs (3 x 2 = 6), there was no significant relationship between rates of divorce and rates of either gonorrhea or congenital syphilis. See Table 1. Thus, for father separation – as indexed by divorce – the hypothesis is not supported.



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**TABLE 1.**

Correlations between percentage of births which are out-of-wedlock and rates of sexually transmitted diseases (from the same time interval), and correlations between rates of divorce and rates of sexually transmitted diseases (from the same same time interval).

| Year | Source of father deprivation | Sexually transmitted disease | N | r<u>p</u> |
|---|---|---|---|---|
| 1991 | Out-of-wedlock births | gonorrhea | 51 | .835 * |
| 1992 | Out-of-wedlock births | gonorrhea | 51 | .786 * |
| 1993 | Out-of-wedlock births | gonorrhea | 51 | .791 * |
| Mean | | | 51 | .804 * |
| 1991 | Out-of-wedlock births | congenital syphilis | 51 | .751 * |
| 1992 | Out-of-wedlock births | congenital syphilis | 51 | .722 * |
| 1993 | Out-of-wedlock births | congenital syphilis | 51 | .784 * |
| Mean | | | 51 | .752 * |
| 1991 | Divorce | gonorrhea | 44 | -.028 |
| 1992 | Divorce | gonorrhea | 47 | .106 |
| 1993 | Divorce | gonorrhea | 47 | -.050 |
| Mean | | | 47 | .009 |
| 1991 | Divorce | congenital syphilis | 44 | -.146 |
| 1992 | Divorce | congenital syphilis | 47 | -.096 |
| 1993 | Divorce | congenital syphilis | 44 | -.253 |
| Mean | | | 47 | -.165 |

* $p < .001$



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*Out-of-wedlock births and gonorrhea (1971-1973 to 1991-1993).*

For all three years, the percentage of all births which were out-of-wedlock births from the prior interval (1971-1973) was significantly and positively correlated to the rates of gonorrhea in the subsequent interval (1991-1993). The mean correlation was also significant and positive ($r_p$ = 933; p < .001; df = 37). In fact, the mean correlation between the *prior* interval (1971 -1973) and the subsequent interval (1991 -1993) of ($r_p$) .933 was higher ($t$ = 5.803; $p$ < .001; df = 36) than the mean correlation between the out-of-wedlock births and gonorrhea from the *same* interval (1991-1993): ($r_p$) .804. See Table 2.

*Out of wedlock births and congenital syphilis (1971-1991 to 1991-1993)*

For all three years, the percentage of all births which were out-of-wedlock births from the prior interval (1971-1973) was significantly and positively correlated to the rates of congenital syphilis in the subsequent interval (1991-1993). The mean correlation was also significant (rg = .871; p < 001; df = 37). Once again, the relationship (re = 871) between out-of-wedlock births and congenital syphilis from the *prior* interval (1971 -1973) was higher (t = 3.701; e < .001; df = 36) than the relationship between out-of-wedlock births and congenital syphilis from the same time interval (1991-1993): p =.752. See Table 2.

*Divorce and STDs: (1971-1973 to 1991-1993)*

For the six combinations of years and STDs (3 x 2 = 6), there was no significant relationship between rates of divorce and the rates of gonorrhea or of congenital syphilis. See Table 2.

## DISCUSSION

Three salient points are available from the data. First, levels of out-of-wedlock births are very strongly related to levels of STDs. Second, levels of divorce are not related to levels of STDs at all. Third, indices of out-of-wedlock births from a pr or generation are more strongly related to levels of STDs in a subsequent generation than are levels of out-of-wedlock births from the *same* time frame as the STDs. These three points are examined below.



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**TABLE 2.**

Correlations between percentage of births which are out-of-wedlock (from a prior time interval) and rates of sexually transmitted diseases (from a subsequent time interval), and correlations between rates of divorce (from a prior time interval) and rates of sexually transmitted diseases (from a subsequent time interval).

| Year | Source of father deprivation | Year | Sexually transmitted disease | N | rp |
|---|---|---|---|---|---|
| 1971 | Out-of-wedlock births | 1991 | gonorrhea | 39 | .940 * |
| 1972 | Out-of-wedlock births | 1992 | gonorrhea | 39 | .932 * |
| 1973 | Out-of-wedlock births | 1993 | gonorrhea | 39 | .927 * |
| Mean | | | | 39 | .933 * |
| 1971 | Out-of-wedlock births | 1991 | congenital syphilis | 39 | .858 * |
| 1972 | Out-of-wedlock births | 1992 | congenital syphilis | 39 | .822 * |
| 1973 | Out-of-wedlock births | 1993 | congenital syphilis | 39 | .858 * |
| Mean | | | | 39 | .871 * |
| 1971 | Divorce | 1991 | gonorrhea | 49 | -.036 |
| 1972 | Divorce | 1992 | gonorrhea | 49 | .024 |
| 1973 | Divorce | 1993 | gonorrhea | 49 | -.033 |
| Mean | | | | 49 | -.015 |
| 1971 | Divorce | 1991 | congenital syphilis | 49 | -.034 |
| 1972 | Divorce | 1992 | congenital syphilis | 49 | -.017 |
| 1973 | Divorce | 1993 | congenital syphilis | 48 | -.113 |
| Mean | | | | 48 | -.054 |

* = $p < .001$



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*(1) Out-of-wedlock births and STDs.*

This exercise has utilized STDs has a marker for multiple sexual partners. To the extent that this equation is valid, out-of-wedlock births are a strong predictor of multiple sexual partners. The basal hypothesis in this exercise is that the presence of an on-going social and biological father affects his developing daughter's psycho-sexual template. She "imprints" upon her father and creates a thematic, masculinized image for herself which she would utilize years later in her post-pubescent reproductive tactics. She shares with the father an affiliative-bond by which she receives nurturing behavior from a trusted adult male. The absence of such a father would result in a more disorganized, less coherent template or image and a more disorganized, less coherent set of post-pubescent reproductive tactics. Her expectations of nurturing behaviors or affiliation from an adult male would be more diffuse. Said more concisely, she would be less likely to achieve an effective pair-bond and more likely to bias toward promiscuity.

*(2) Divorce and STDs*

There was no found relationship between rates of divorce and rates of STDs, i.e. multiple sexual partners. This finding seems odd, given that levels of out-of-wedlock births are so strongly associated with the STDs, and that both out-of-wedlock births and divorce define an absence of the father. An attempt at a partial explanation of the difference between the effect of out-of-wedlock births and lack of effect by divorce is given below.

*Out-of-wedlock births versus divorce*

In the U.S., an out-of-wedlock birth, with few exceptions, precludes the father from residing with his biological child. As a matter of contrast, a divorce, again with a few exceptions, indicates that the child – the daughter – has resided with her father for some time period and then was separated from her father. Although there are certainly potential variables of personality-types and SES and local traditions *inter alia* which would suggest that the sample of people who divorce could be distinct from people who never marry, but have children, one particular variable is of interest for this exercise. To wit: the presence of fatherhood. Fathers are absent in



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out-of-wedlock births, but fathers are temporarily present prior to a divorce.

Survey data in the U.S. which address "divorce" rarely include age of the child at the time of divorce. One serendipitous exception is the work of Morgan, Lye, and Condran (1988). Using data from the Current Population Survey, they coded age of the first born child at the time of divorce. They coded over 90,000 children. Of interest here is the age of daughters at the time of divorce.

For daughters who were singletons, 42.4% of the daughters resided with their fathers for at least their first three years of existence prior to the divorce. For the first-born of two daughters (only), 90.6% resided with their fathers for at least their first three years prior to the divorce. For two children (only) – one of each gender – 92.1 % of the first born were with their father for at least their first three years. Using the assumption that approximately half of these children were females, then 46% (92.1% x .5 = 46.05%) of these children were daughters. Thereby, a large number of the daughters resided with their fathers for several years in their early development before the father and the child were separated by divorce. See Table 3.

It is important to note that U. S. mothers, by over a 2 to 1 ratio, are the petitioners in a divorce situation involving a minor child. The ratio is approximately 1.6 to 1 if no minor children are involved in the divorce proceedings. (National Center for Health Statistics 1996). (See Buckle et al [1996] and Sullivan and Allen [1996] for similar trends in countries other than the U.S.). Thus, the man is typically abraded from his child – his daughter – not by his own volition, but by the choices made by the mother and by the laws of the states.

Because all states plus D.C. have some version of "no-fault" divorce, if a spouse (usually the wife) petitions for a divorce, the divorce will occur. The respondent (usually the father) cannot contest the divorce. Custody of the child can be contested, but, in spite of *de jure* pronouncements of gender neutrality (Mcintyre and Sussman, 1995, Westfall 1994, Weyrauch and Katz 1983), the mother *de facto* receives custody in a large majority of the cases (Sitarz 1990, Sack 1987). In 1994, more children were living with neither parent (4%) than with the father-only (3%) (69% were living with both parents and 24% were living with mother-only) (U.S. Bureau of the Census



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1996). Once the father is separated from his daughter, contact between the two is generally low and is consistently lessened over time (Dudley 1991, Furstenberg et al. 1983, Furstenberg et al. 1987, Kruk 1994, and Seltzer 1991, c.f. Braver et al.1991).

Thus, it is suggested that the early and consistent contact between a daughter and her biological and social father serves to "set" her psycho-sexual template, which – years later – influences her reproductive tactics. In this instance, it is the number of partners which is the focus. An out-of-wedlock birth effectively precludes such contact. A large proportion of divorces do not preclude such contact and thereby allows the dynamics to occur.

*(3) A generational lag: out-of-wedlock births and STDs*

Even though the levels of out-of-wedlock births were significantly and strongly related to STDs (again, a marker for multiple sexual partners) during the 1991-1993 interval, knowledge of out-of-wedlock births in the *1970s* was the superior predictor of STDs in the *1990s*. This enhanced predictability proffers inferential evidence that once fatherlessness is instituted in a community, it may be difficult to re-instate social fatherhood. In other words, mothers who were without social fathers would tend to have daughters who would be less likely to achieve a functioning pair-bond (Morrison and Cherlin 1995). In addition, mothers who give birth out-of-wedlock have a comparatively lessened chance of marrying (while their children are minor) (Aqullino 1996; Bennett, Bloom, and Miller 1995). The daughters would not remain chaste, but would turn the cycle one more rotation by having daughters of their own who, too, would grow up without a co-residential, biological and social father.

## CONCLUSION

This exercise argues for two points. First, the presence of a social father with his daughter, during her formative years, affects the developing psycho-sexuality of his daughter. The suggestion is that this early influence lends itself to an easier achievement of a successful pair-bond by his daughter-grown-to-maturity. Second, there seems to be no model or theory (at least easily findable) which gives a synthesis to a/any unique father-daughter affiliation. In what is prob-



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**TABLE 3.**

Percentage of children involved in a divorce by age of child and by birth order at time of divorced (adapted from Morgan, Lye, & Condran [1988])

| Number and gender of children | Age of first born child (in years) at time of divorce: 1 | 2 | 3 or more | Total | N |
|---|---|---|---|---|---|
| One girl | 34.3% | 23.3% | 42.4% | 100.0% | 16,641 |
| Two girls | 0.8% | 8.6% | 90.6% | 100.0% | 8,671 |
| Two children: mixed | 0.5% | 7.4% | 92.1% | 100.0% | 19,277 |

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ably fairly rare in the behavioral sciences, the above exercise has led to statistically significant results – empirical realities – in search of a synthesizing theory.

Bowlby's (1973, 1982, 1988) theories really are based on monotropy and not easily transferable to the father-daughter dyad. Freud's (1925, 1931, 1933) theory on the *Electra Complex*, despite Freud's own discomfort with his own formulations, at least begins the analysis, but such an analysis is probably in need of a hefty refurbishing. Much of the dynamics of extant/traditional theories is based on a mother paradigm or a gender-neutral paradigm (Craik et al. 1993, Maddi 1980, Ross 1992, Schultz and Schultz 1994), neither of which would be able to explore the putative and evolutionarily based uniqueness of the father-daughter affiliative bond. Similarly, neither Barkow, Cosmides and Tooby (1992) nor Crawford, Smith and Krebs (1987) addresses father-daughter affiliation. Nonetheless, it seems that there would be much to be gained if such a developmental theory or model on the father-daughter relationship could be assembled in the near future.

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# A Humor Workshop Program to Aid Coping with Life Stress

Michael J. Lowis
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Following a brief review of some uses to which humor has been put, the author describes the development of a Humor Workshop program to assist people in the use of humor as a coping mechanism for life stress. Volunteers, arranged in three small pilot groups (N = 22), attended the complete course of four sessions of structured learning, plus a fifth one for retrospective discussion. Analysis of quantitative data revealed a significant increase in the use of coping humor over the duration of the Workshops, and a scrutiny of qualitative feedback and participant's comments yielded pointers to how it might be improved for future presentations. An intervention strategy based on the creative use of humor would appear to have potential value for the maintenance of mental health and well-being.

KEY WORDS: Humor, Life Stress, Psychotherapy.

Although the topic of humor has been debated at least from the time of Aristotle in the fourth century before Christ (Banbraugh 1963), for much of recorded history significant contributions on its use have been somewhat infrequent. During the 1960's, however, a surge of interest in this field of study occurred (cf. the *Humor Movement,* Korotkov 1991), with the realization that humor and laughter serve important roles in maintaining and enhancing the quality of life.

It has been speculated that the overt phenomena of laughter and smiling had their origins in expressions of victory and defeat respectively, even before human beings as we know them had evolved from their earliest hominid beginnings (Pettifor 1982; Raskin 1985). It is thus posited that from early in hominid evolutionary history manifestations of humor-related behavior served specific purposes such as communication and the release of emotion.

Ideas concerning the usefulness of humor can be arranged into four basic categories. Firstly, there are those involving social facilitation and control as summarized by Overholser (1992). Humor



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can help build relationships and maintain social bonds, as well as ease tensions and reinforce the norms of the "ingroup" – partly through ridiculing the "outgroup."

The second category of humor use is in the sphere of teaching and education. Rutkaus (1981) notes that long-term retention of subject matter is enhanced when humorous examples are included in the program, while Dolce (1984) found that encouraging people to laugh at their mistakes produced positive benefits in the classroom.

Thirdly, humor can be used as an expression of veiled aggression as also in mediation - this use being found among court jesters and ritual clowns of the Middle Ages (Lieberman 1977; Haig 1986). The modern counterparts of such performers are comedians (particularly those involved in political humor: Benton 1988) and circus clowns.

The fourth and final use of humor is the one with which the present article is most concerned, namely as an aid to coping with stress and in psychotherapy. Such benefits were obviously known to at least one writer of the Old Testament, who stated: "A cheerful heart is good medicine, but a crushed spirit dries up the bones" (*Proverbs*, 17:22, New International Version). In more recent times Freud (1952, originally published 1928) recognized the therapeutic value of humor, commenting that "... it takes its place in the great series of methods devised by man for evading the compulsion to suffer ..." (p.217). His theory, discussed in more detail in an earlier work (Freud, 1976 - originally published in 1905), was that through humor people reduce their problems to "child's play", but that it was a "rare and precious gift" that few can actually achieve.

Levine (1977) presented an overview of the understanding (at that time) of some of the useful roles played by humor, and it is clear that the topic was being accepted as one worthy of serious study. Among the points made by Levine was the belief that a humorous attitude helps one feel more capable of overcoming fears and tolerating distress; it could relieve anxiety and re-assert ego mastery. He was of the opinion that by 'whistling in the dark' a person who may be experiencing anxiety introduces a non-serious activity into the situation. The feeling of uneasiness has difficulty competing with this jocularity; with the result that it is often dispelled.

The consensus from the minor flood of pertinent references which have subsequently appeared, is that humor can be effective in



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stressful circumstances by introducing light relief through the adoption of a playful mindset, and from the sheer pleasure of cognitive mastery.

The key to success appears to be in the creative ability to bring multiple perspectives to bear on real or perceived problems, aided by the promotion of objectivity through distancing oneself from the situation. This is not to say that problems are reduced to trivia, but that when they can be viewed from different angles a variety of solutions usually present themselves and these ultimately increase the chance of successful resolution (McGhee 1979; Lefcourt and Martin 1986; Linstead 1988; Overholser 1982). A useful 20-year retrospective review on humor in counselling and psychotherapy is given by Shaughnessy and Nadsworth (1992).

The main thrust of the present article is to describe the development and piloting of a Humor Workshop program designed to assist people to use humor as a coping mechanism for life stress. Quantitative data of limited scope, together with content analysis of feedback from those who attended the sessions, are used to evaluate the effectiveness of the program.

**Training in the Use and Production of Humor**

Although there are numbers of references to the use of humor in counselling and formal psychotherapy, none have been noted which specifically report on training through a workshop format such as is adopted in the present study. This does not preclude the possibility that therapists interested in humor techniques may have conducted sessions with groups on the use of humor as a coping mechanism. Roller and Lankester (1987), for example, make reference to incidences of spontaneous laughter in group therapy sessions conducted for depressed patients.

With regard to advice on the techniques of humor construction, Freud (1905-1976) lists the procedures involved in 'joke work'. These include: condensation (making a conical word out of two others e.g. 'anecdotage'), multiple use of the same sounding words (eg. 'buona parte' in a joke involving Italian for both 'a good part' and Napoleon's surname), and double meanings of words (eg. 'double entendres'). A two-stage theory of incongruity-resolution was introduced by Suls (1972), where a joke or cartoon was intentionally



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constructed so as to lead the recipient astray and produce surprise. The more the punch line violates the listener's expectations, the more the joke is likely to be appreciated.

In developing this theme, Shibles (1978) mentions several ways of creating humor, these being based on deviation from what is expected, familiar, probable, or reasonable. A further contribution stems from Mulkay's (1988) discussion on the earlier work of Sacks. Standard jokes often make use of a three-part sequence: the first acting as a guide for what is to come in the second, whilst the third breaks the pattern established by the first two. This is typified by many jokes involving three characters: the first two behave in similar fashion whilst the third does something different, but nevertheless relevant, in a novel way. Note that additionally such jokes often involve cultural stereotypes, as in the Englishman, Irishman, Scotsman versions.

Finally, a step-by-step guide to using humor in business speaking was compiled by Michael Iapace (1988), a comedian and writer of joke material. Among the many tips he gives is a way of "switching" old jokes with regard to either the set-up (subject or location), punch line, concept, time frame, or even the whole point. After suitable polishing, this can result in witticisms that have the appearance of originality.

## The Humor Workshop Program

*Content of the Workshop sessions*

Five sessions were devised, each of approximately two hours duration. The first four comprised instructional or experiential learning, and the fifth was largely a review and semi-structured discussion. In general, a balance was sought between imparting interesting information, encouraging group dissension, and practical exercises such as role plays and feedback of homework assignments. A short summary of the contents of each session is as follows:

*Session 1* General welcome; cartoon-rating exercise (part of a separate study, but for Workshop delegates it provided some amusement and relaxation); brief overview of the program; the nature, causes and consequences of stress, both mental and physical;



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the message of humor being linked to creativity (providing alternative perspectives on a problem and, thus, a choice of coping strategies); exercise on creative thinking (uses for a solitary object); first lesson in humor construction (Suls 1972; theory of incongruity resolution; Mulkay 1988, three-part joke sequence); homework exercise: creation of witty responses to given statements.

*Session 2* Free discussion on participant humor attempts and experiences during the past week; presentation of homework assignments; brief history of humor and laughter; exercise in self-deprecating humor (each person present tells an amusing anecdote about themselves); exercise in reverse-role plays to see the other person's point of view (participants adopt roles of opposite sex, and debate gender superiority from this new perspective); second lesson in humor construction (creation of witticisms by deviating from the normal rules of language and expression); homework exercise: creation of nonsense phrases as per above.

*Session 3* Free discussion; presentation of homework exercises; overview of humor theories; creation of "spontaneous" wit (puns, exaggerations, *double entendre*, etc); third lesson in humor construction (certain rules of joke construction, and the need to be topical); homework exercise: preparation of short, witty speech incorporating the techniques learned to date.

*Session 4* Free discussion; presentation of homework speeches; fourth lesson in humor construction (the switching technique of Iapoce 1988); role-playing exercise where participants adopt a disability (short, tall, wooden leg, etc) and relate amusing stories of imagined experiences; summary of all sessions to date; homework exercise: creation of switched jokes using the techniques discussed.

*Session 5* (Held five weeks later). Review of Workshop objectives and the ground covered; semi-structured discussion on participants original expectations, what they had achieved, and how the program could be improved. The discussion was recorded on audio tape for subsequent content analysis.



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**Participants**

Participants for these pilot groups comprised those who responded either to announcements made through a church in the South African town of Benoni, or to an editorial comment in the local newspaper. The invitation was to people who had experienced a stressful event in the past year, who felt they lacked assertiveness, or who believed that they were not coping well with life in general. It was specifically mentioned that the topic would be humor. There was no requirement that participants should belong to the church in question, but only that they should be aged 18 years or older (to have acquired some level of maturity). The Workshops were not targeted at individuals who were in the acute stage of mental trauma.

In order to create small, interactive classes the initial pool of 26 applicants was divided into three groups. One person failed to arrive and three dropped out, leaving 22 who completed all the sessions There were 13 females and 9 males, including 8 married couples. The age range was 25 to 71 years, with a mean of 48.7.

**Procedure**

Three pre- and post-intervention instruments to measure the humor variable were administered to those who participated. These were (i) the Humor Response Scale (HRS) (Lowis 1993), comprising 36 cartoon items which have to be rated for funniness; (ii) the Coping Humor Scale (CHS) (Hartin and Lefcourt 1983; 1984), which has seven items designed to measure the degree to which subjects report using humor as a means of coping with stressful events; (iii) the frequency of Humor Initiation (HI) scale (Bell, McGhee and Duffey 1986) of just six items, designed to assess how often an individual makes the effort to generate some form of humor. In addition, the Affect Balance Scale (ABS) of Bradburn (1969) was given as part of the pre-test battery. This 10-item instrument measures both positive and negative feelings, the results being expressed as positivity of affect.

Four hypotheses were formulated relating to anticipated changes brought about by the Workshop program. These predicted that scores on the first three instruments would significantly increase, and that individuals who make significantly more use of coping humor after attendance, will initially have had the lowest levels of positive



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TABLE 1

COMPARISON OF SCORES FROM MEASURES ADMINISTERED BEFORE AND AFTER THE HUMOR WORKSHOP PROGRAM (N = 22)

| Measure | Mean before | Mean after | t-test | p-value |
|---|---|---|---|---|
| Humor Response | 115.05 | 119.14 | .350 | .364 |
| Humor Coping | 10.73 | 12.86 | 1.922 | .031 |
| Humor Initiation | 5.23 | 6.68 | 1.259 | .108 |



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affect.

The first four sessions were designed to be held at weekly intervals, either on Saturday mornings or mid-week evenings. An interval of four weeks was allowed before the fifth and final session in order for participants to have the opportunity to put into practice what they had learned from the program.

Data for assessment of the course effectiveness were obtained from the pre- and post-intervention measures, semi-quantitatively from a simple session rating form from each participant, and qualitatively from content analysis of comments from each session and from the major discussions recorded at the final meetings.

**Results and Discussion**

*Statistical analyses* The results of t-tests, computed from scores on the three measures before and after attendance by the 22 people, are summarized in Table 1.

One-tailed probability estimates were used, because all the applicable hypotheses were directional It will be seen that, whilst scores on each of the post-intervention measures are higher than those obtained initially, this increase is statistically significant only for the use of coping humor.

The increase in the use of coping humor was further investigated with regard to levels of positive affect.

A *Pearson r* statistic computed between ABS scores (initial positivity of affect) and changes in the CHS scores (post-test minus pre-test) yielded a correlation of .381 ($p = 08$). This finding falls just short of statistical significance on levels normally employed. However, the suggestion that those individuals who were the most depressed prior to attending the Workshops may have benefited by increasing their use of coping humor more than did those who were initially less depressed is worthy of further investigation. This would reveal something about the candidates who can potentially benefit the most from Humor Workshop intervention programs.

*Session ratings*

Because these workshops were of a pilot nature, an indication of their enjoyment and effectiveness was sought from rating forms completed by participants after each session. These contained three



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questions relating to how interesting the particular Workshop was, how often an attempt was made to put what was learned into practice during the week, and how much benefit was obtained from such applications. The responses were recorded on a forced-choice Likert scale, and the mean ratings of each group for every session were computed.

These data are not suitable for statistical analysis, but the trend was for scores to be higher at the end of the completed program than at the beginning, but with peaks being near to the middle. During the sessions it was indeed noticed that participation, spontaneous banter, and group cohesiveness did seem to increase as members warmed to the program.

**Qualitative Feedback**

Data based only on group averages do not always reveal the total picture, and it was never expected that the Workshops would be equally beneficial to all participants. From a content analysis of the comments from each of the 22 individuals who completed the program, three categories of benefits could be identified. Five persons were judged to have obtained high benefit, nine moderate benefit, and eight low or zero benefit. Some features of each of these categories are given below.

*(1) High benefit*

Three of the participants (two female and one male) judged to be in this category started the program with severe or moderate depression (ABS score), but reported that they were not currently experiencing acute traumatic stress. It seemed that they were either recovering from some past trauma, or were suffering the consequences of chronic stressful circumstances. Interviews conducted with two of these individuals some six weeks after completion of their course indicated that the knowledge and skills acquired during the Workshops were still helping them cope with their particular life situations.

The remaining one man and one woman appeared superficially to have adequate ego strengths and other life skills, but they nevertheless showed high gains in the use of coping humor. Seemingly they were receptive to the message of the Workshops and perhaps



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had some inner need, unrelated to their level of stress, to increase raise their humor skills.

*(ii) Moderate benefit*

The six women and three men in this category showed a wide variation in their trauma history and current affective states, as well as in their level of participation during the sessions.

Some mentioned that they enjoyed observing the interaction among other members of their group, being content themselves to stay largely on the sidelines. One woman came with a very depressed affect due to the consequences of a divorce, and was currently having problems with her children. She did not contribute much to the sessions (although her command of English was adequate, her home language was German), but stated in the final session that she could now respond more positively to her children. Perhaps this person needed a stronger form of therapy than the program had to offer.

*(iii) Low or zero benefit*

The four men and four women (including three married couples) who were assessed as receiving the least benefits, appeared to fall into one of two groups. Either they did not participate in the activities, and demonstrated low overt enthusiasm, or they had such well-developed ego strengths and coping skills that they did not need any help in this regard. Nevertheless useful contributions to the sessions were often received from these people, including suggestions as ro what should be retained or discarded from the program. One woman reported that she had tried to apply more humor in her everyday life, but that this was unsuccessful. The participant with the least measurable benefit was actually a very consistent contributor, frequently offering anecdotes and commenting on the program content.

**Conclusions**

When the Humor Workshops were designed, it was believed that the main therapeutic benefits would stem from the use of creativity to bring fresh perspectives to bear on problems and generate alternative solutions. Once free of the restriction of seeing a situation in only one light, a person's difficulty should decrease in gravity and



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he or she should then be able to view it with a degree of detached humor. However, additional uses for humor were suggested by the participants themselves. The notion that humor can permit a period of relief from stress (recharging the batteries), assist in recovery from a crisis even though it did not help at the time, and create the opportunity for feeding remarks into a conversation that others can turn into witticisms, were among the useful items volunteered during discussions.

It is clear that at least some participants did obtain significant benefits from the exercise, but the fact that 36% failed to gain any measurable improvement in coping skills indicates a need to review the target population for this program. The most suitable candidates for Humor Workshops do, however, remain close to that originally specified (but not to which all participants complied): namely those who had passed through an acute period of stress and trauma and who needed coping skills to help them overcome possible longer-tern depression. This notion is supported by the indication, albeit in need of further study, of a link between initial negative affect and an increase in the use of coping humor.

Although care should be taken to attract volunteers who comply with the specified criteria, it might be impractical to exclude others who wish to attend. Perhaps it would be advantageous – as one of the participants suggested – to include some optimistic, well-adjusted individuals in the groups. Such people might, by their mien or approach to life, exemplify the benefits of humor as a coping mechanism and help conversation with the uninitiated.

Previous workers have noted the positive effects humor has on those suffering from depression. Roller and Lankester's (1987) observation of a fine sense of humor among depressed outpatients, led the authors to conclude that this served the function of self-activation and social contact with others at a different emotional level. Bischoff (1991), whilst not finding that humor actually decreased depression induced by experimental manipulation, did conclude it to be linked to a positive re-interpretation of circumstances and the ability to face stress in a more positive manner. Additionally, Overholser (1992) showed that humor was associated with lower depression and higher self-esteem, especially with female subjects.



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The present study was, by its nature, a pilot exercise conducted with a relatively small number of participants. Conclusions must therefore be treated with caution at this stage. Nevertheless there do seem to be indications that humor intervention programs have potential as an effective tool for helping people cope with the effects of stress and depression. Mental health professionals can make use of a strategy such as that outlined in this article for a variety of interventions. These could range from workshops offering training in coping skills in potential stressful situations (defense or resistance building), through regular top-up sessions for those in high stress occupations, to mild therapy for sufferers of moderate stress or to aid recovery from earlier traumas. It is believed that an improvement in well-being for people in a wide variety of domestic and occupational situations could thus be achieved.

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*BOOK REVIEW ARTICLE*

# African Exodus - Re-Evaluating an Hypothesis

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**African Exodus: The Origin of Modern Humanity**
*Christopher Stringer & Robin McKie*
Henry Holt, 1997

That the authors of this book are from two different backgrounds suggests there may be some tension in the book. Christopher Stringer (paleoanthropologist at London's Natural History Museum) is a well known scientist, but Robin McKie is a journalist (science editor of *The Observer*). *African Exodus* is a politically correct history of mankind, devoted to defending the Out of Africa theory of human origins, and incidentally arguing that therefore all humans are basically identical. One can speculate that Stringer supplied the basic facts while McKie was responsible for the political gloss.

The organization of the book is chronological, tracing human history from its origins. However, the first chapter is autobiographical, recounting the Stringer's first work on human skulls (done as part of his PhD thesis). It then describes how the Kibish skull from Ethiopia looked modern to him. Yet it dated to about 130,000 years ago (i. e. when Europe was inhabited by Neanderthals with quite different looking skulls). This led him to develop the Out of Africa hypothesis that modern humans had evolved in Africa, probably in East Africa, some of whom later moved out of Africa to populate the world.

This chapter is followed by one arguing for the East African ancestry of modern humans, and then a couple of chapters on the Neanderthals and their replacement by Cro-Magnons. The next two chapters deal with the genetic evidence on human evolution and how that points to an African origin for humans. A chapter is devoted to

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the apparent intellectual advantages of the Cro-Magnons, and speculation as to just what they had that the Neanderthals lacked that led to their replacement.

The part of the book devoted to reviewing hominid origins appears competent and is certainly very readable. It differs little from other popular accounts. It has the advantage of being more up to date than older ones, carrying the evidence of research into 1996. For instance, it provides a vivid account of the discovery of a Neanderthal infant by Rak (for a scientific description see Rak, Kimbel, & Hovers, 1994), seemingly too different from current *Homo sapiens* infants to be the same species.

In one case, an interview brings the story more up to date even though the published work was not yet available. The genetic work of Tishkoff and Kidd on a CTTTT repeat on chromosome 12 is described. It provides very powerful evidence for an African origin of the hominids. (A published report is now available in Tishkoff, et al. 1996). Of course, there is always new genetic work, too new to be cited, notably that concerning the Y chromosome (Zerjal, et al. 1997 is a recent example).

Unfortunately, the book went to press too early to give the important finding (Krings, Stone, Schmitz, Krainitzki, Stoneking, & Paabo, 1997) that Neanderthal mitochondrial DNA is sufficiently different from modern humans to make modern Europeans descent from Neanderthals unlikely. This finding greatly weakens the case for any multi-regional hypothesis of human origins in the form advanced by its contemporary exponents, when they argue that Cro-Magnons evolved from Neanderthals. Since the book went to press further evidence suggesting the Neanderthals were not the ancestors of current Europeans has appeared (Richards, et al., 1996, Schwartz & Tattersall, 1966, Holliday, 1967, Armour et al, 1996).

The major errors in the book arise when it moves beyond paleoanthropology and attempts to be politically correct. I suspect McKie (a journalist) may have written these sections, although a co-author such as Stringer certainly deserves to share the blame for obvious errors. Stringer is the author of another recent book on a similar subject (Stringer & Gamble, 1993) which seems free of the political correctness problems of *African Exodus*.

*African Exodus* digresses from paleoanthropology (Stringer's area



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of expertise) to attack *The Bell Curve* (Herrnstein, & Murray, 1994). It makes the ignorance of Stringer and McKie apparent by attributing to *The Bell Curve* statements it does not contain.[2] One strongly suspects that neither of the authors have actually read the book they attacked - an embarrassing situation. They do provide a quote (p. 190) from Beardsley's critical review (which itself implies *The Bell Curve* contains statements it does not make), and this may be the source responsible for their erroneous statements. We are to believe that Stringer has carefully examined the evidence provided by various prehistoric fossils and made difficult interpretations, while being supplied with evidence that he failed to read the clear words in a book he discusses, or as a minimum, relied on a biased or careless journalist co-author.

*African Exodus* caricatures J. Phillipe Rushton's work. The authors claim that "Prof. Rushton simply assumes that the Out of Africa theory establishes the primitiveness of Africans and the superiority of Orientals" (p. 184). They then go on to say "It does nothing of the sort, of course." The only problem is that Rushton says nothing of the sort. A reading of his work (including his uncited book *Race, Evolution and Behavior*, 1995), where his thinking is most clearly set out, will show that his thinking, right or wrong, is much more sophisticated than that. Rushton provides evolutionary reasons (which they state on p. 184, before immediately forgetting them and making the above accusation) concerning the nature of the equilibrium that would emerge in different environments.[3] It is not sensitive to where the organism originated, or to the exact evolutionary path followed, and would be little altered if humans had originated in Asia, or if some multi-regional theory of the human

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[2] For instance (p. 185), they accuse Herrnstein and Murray of saying that no more state or national aid should be lavished on the problems of American blacks, a recommendation that is not made in *The Bell Curve*. Later (p. 190) they write "The authors of *The Bell Curve*, and all its apologists, argue that putting resources into education to improve the intellectual performance of children from deprived backgrounds is a waste of money." Such a statement is not to be found in the book they are criticizing. For those curious about what is really in *The Bell Curve*, a review and summary has been published in this journal (Miller, 1995a).

[3] Certainly Rushton's arguments can be critiqued on several grounds, such as that a simple climate-based theory better explains the data (Miller 1993, 1994a, 1995b).



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origins were accepted, although he does usually present his argument in terms of an Out of Africa scenario.

Thus the authors of *African Exodus* are wrong (p. 245) when they say "We have seen how Rushton has used our theory to account for the claimed primitive intellects and behavior of Africans." If the authors of *African Exodus* have not bothered to read Rushton's theories in detail (and the citations indicate they have not), they really should not be taking credit for them.

Again the problem may be the result of a journalist relying on secondary sources. *African Exodus* cites some popular discussions of Rushton's work (a *Rolling Stone* article of all things), and a 1989 lecture of his, but fails to cite Rushton's well known and readily available book (1995b, now in paperback, Rushton, 1997) or his many scientific papers dating from as early as 1988 (Rushton 1988). One would think that if one is devoting several pages to attacking someone, the references would at least indicate where the latest and most complete version of his views could be found. A paper in the journal *Society* is mentioned (Rushton, 1995a), but not the papers in the professional literature. The most plausible reason for the absence of more citations to Rushton's professional works is not having read his latest scientific publications, and instead relying on popular and secondary sources.

The authors also devote some time to attacking the idea that brain size and IQ might be related (even after devoting part of the book to recounting the growth of hominid brain size over time) in spite of the number of studies that show such a relationship (see Rushton & Ankney, 1996 for the long list). They describe (correctly, p. 230) the problem today's human females have in giving birth to babies with large heads, but do not recognize that evolutionary theory predicts that there must be some advantage derived by the large-brained contemporary humans (greater intelligence is the obvious candidate), or otherwise the higher death rates for mother and child during childbirth would have prevented them from evolving such large brains.

The basic political argument of the book is made clear (p. 190): "In any case, the story of our *African Exodus* makes it unlikely that there are significant structural or functional differences between the brains of the world's various peoples." Even stronger is (p. 161), "It



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is, after all, a specific prediction of the Out of Africa theory that racial characteristics are new and relatively unimportant facets of our species' anatomy." If this statement were taken seriously, the psychometric data (and the observations on skin color) would serve to disprove the Out of Africa theory, but the logic here is wrong.

Even should one accept the authors' argument that as little as 100 millennia have elapsed since the ancestors of Europeans and Asians left Africa (and on p. 159 they give 120,000 years for that date), that length of time equates to 5000 generations of 20 years each. To produce the 15 IQ point difference observed between living sub-Saharan Africans and Caucaoids/Mongoloids, those that left Africa merely need have their IQ rise by an average of 0.003 IQ points per generation – a modification which one could easily imagine as being produced by stronger selective pressure for intelligence in the new climatic and ecological environments to which the migrants were exposed. It is clear that there is considerable genetic variability in humans in the genes affecting intelligence. Such selective pressure could either be due to intelligence making a greater contribution to survival in the more challenging ecological environment associated with colder climates (see Miller 1991) or to social forces making intelligence more important to reproductive success in some areas (Miller 1995b).

Any theoretical argument that evolution could not produce certain types of differences is very weak, and should not be given much weight when there is empirical evidence that differences exist. *African Exodus* itself discusses the evidence that brain size differs with climate (Beals, Smith, & Dodd, 1984) and race, but argues it is related to the advantage that larger brains (and larger bodies) have in keeping warm in cold climates (p. 186), rather than to the greater contribution of intelligence to reproductive success in cold climates. Of course, even if the larger brains resulted solely from any advantage they possess in retaining warmth, it is still possible for differences in racial intelligence to have resulted from such climate induced differences in brain size.

As I pointed out in this journal in a paper on Australian intelligence (Miller 1996), there is evidence not only of poorer performance on intelligence tests by Australian aborigines, but also of differences in brain size and anatomy (Klekamp, Riedel, Harper,



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Kretschmann, 1987; 1994). The strong evidence that races differ in intelligence has been reviewed by Lynn in this journal (Lynn 1991a), along with a discussion of possible causes (Lynn 1991b) and a more recent review by him has just become available (Lynn, 1997).[4]

The *African Exodus* discussion goes on (p. 191) to discuss the Human Genome Diversity Project, quoting with approval Dunston's statement (made in *Discover* magazine) "After the diversity project we won't have the luxury of drawing distinctions between one another based on skin pigmentation," something that cannot logically be expected from a project that studies only a limited selection of stretches of our DNA, none of which are known to have any relationship to intelligence, to see how these vary among different populations. There is already a massive amount of data on genetic variation among peoples, which has been summarized recently in a massive work (Cavalli-Sforza, Menozzi, & Piazza, 1994). However, as was pointed out in a review and summary in this journal (Miller, 1994b) all this massive data, when shown in the three color map on the book's cover, shows that humans can be divided into races on the basis of the genetic data included in the survey – none of which includes genes relevant to traditional anthropomorphic racial classification. These groupings correspond remarkably closely to the traditional divisions made on the basis of pigmentation and outwardly visible morphology. In spite of what *African Exodus* implies when it puts terms like "Caucasoid" in quotation marks (p. 65), the traditional

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[4] Rather than directly confront the empirical data on racial differences in intelligence, *African Exodus* tries to argue that the length of time since *sapiens* hominids left Africa is too short for differences to have arisen. Rather than reviewing the empirical evidence that *The Bell Curve* and other sources have aggregated on racial differences (citing solid research statistics), it merely notes (p. 190) that on the day *The Times* carried an account of *The Bell Curve*, it also carried an obituary for a certain Davidson Nicol, an African who had had a distinguished academic and diplomatic career, as if the existence of a single African of high IQ could disprove a solid statistical case. Their closest to statistical evidence is to cite a newspaper report that in contemporary Britain, Black Africans are twice as likely to hold jobs in the professions as white people. Given the British immigration rules that give preference to the educated, this is not surprising, and does not disprove the documented evidence of racial differences in average intelligence. Even in Britain, the bulk of the Negroids (immigrants from the Caribbean) do much worse in school than the native born Caucasians.



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racial schema emerges once again when classification is based on modern genetic data.

Gene flow from one region into another would serve to retard the evolution of regional differences. But Stringer and McKie make a point (p. 144) in their attacks on the multi-regional theory of human evolution of emphasizing the geographical barriers to gene flow, and how prehistoric breeding populations were probably geographically isolated. They also emphasize the barriers to interbreeding caused by populations looking different (p. 145), although later in the book they cite approvingly Brace's denial that people noted that human populations looked different before the age of discovery (p. 191) – a statement which is quite ridiculous in view of the still surviving disaparities of appearance that distinguish living human populations.

Of course, in other parts of the book the possibility of evolution producing changes over that time period is recognised. There is a fascinating discussion of how many modern Englishman and others have jaws too small for their teeth, leading to problems with wisdom teeth, and how this had led to some human having fewer than the full compliment of 32 found in other humans. The book (p. 228) states "Among Europeans, for example, it has been found that up to 15 percent of people have at least two wisdom teeth missing .... while in east Asia, the figure can be as much as 30 percent in some areas." The authors also describe how before modern medicine, an impacted wisdom teeth could became infected and lead to death, citing this as an example of evolutionary selection.

The account of the evolutionary pressures refers to "white breads, pot noodles, pizzas, and all the other low-chew, takeout wonders of modern cuisine". Of course, these are quite recent developments. Yet one can hypothesize the decline of jaw size as having begun with the consumption of boiled foods (for which there is early archaeological evidence in Europe and Asia in the form of pottery).

Thus, the authors seem to find it plausible for evolution to act through differential death rates resulting from differences in the number of wisdom teeth (surely a weak effect), yet find it implausible that the challenge of living in a new and severe environment could result in a selection in favor of higher intelligence. Consider the consequences to a group that was not smart enough to build a fire from scratch when lost in the winter. This is without mentioning the



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advantages of intelligence in a cold climate, where provisioning is important, in helping detect whether one's mate is cheating, or is likely to cheat (see Miller 1995b) – a form of selection for intelligence that may have more impact on reproductive success than differential survival.[5]

The authors discuss (p. 217) the problems women weighted down with children faced as their tribe roamed through foraging territories or followed herds of reindeer or caribou, and the resulting needs for male provisioning. However, it fails to recognize that the need for male provisioning would be much greater in the cold climates where males supply virtually all the food than in tropical climates where most food is female supplied.

The authors similarly mention how noses have been shaped by climatic differences in different regions (p. 227) and refer to differences in resistance to skin cancer that arise from the races having evolved in different regions, and the possibility of future evolution from this (p. 245). They seek to show how racial differences in physical attributes could have emerged in as little as 100,000 years (or what ever time the races are argued to have been separated) and see no problem in this.

Yet in discussing intellectual differences that appeared with Cro-Magnon man, the authors remark that (pp. 196-197) "If we accept that neurological or behavioral changes were responsible for the abrupt flowering of human culture only about 40,000 years ago then we have to explain how this moment of transformation occurred more or less simultaneously across Africa, Asia and Europe. As we have seen, the DNA studies by Harpending, Rogers, and others demonstrate that a whole variety of people – Turks, Sardinians, Australians, Japanese, Native Americans, and others – all went through sudden and rapid eruptions in populations at the same time that our mystical Cro-Magnon creative explosion was occurring" (the reference is to Harpending et al. 1993). The genetic evidence is much too weak to justify using the word "simultaneously." For instance, Sherry et al. (1994) state, "Although the African expansion appears

[5] For a further discussion of implications of evolution in different areas and the policy implications, see Levin 1997.



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to be the earliest, the large confidence intervals preclude any definitive ordering of regional expansion times." This also precludes any statement that they were simultaneous.

The error brackets on the determination of expansions from genetic evidence are very large and cannot provide evidence for "simultaneous" events. For instance, a recent application of mismatch analysis is Bortolini et al. (1997), and they conclude "The mismatch distribution obtained here suggests the the major episode of human population expansion occurred between 24,836 and 94,887 years ago. . . the large confidence interval, however precludes any definite association between this event and the human technological development that occurred 50,000 years ago."

Admittedly, the above quotation from *African Exodus* is followed immediately by "Clear evidence for rises in human numbers and in artistic and technological sophistication has only been found in Europe, but then – paleontologically speaking – this is the world's most assiduously studied continent." This sentence is correct. They should have stopped here.

The problem with *African Exodus* is that it is politically difficult to discuss what may have produced a flowering of intellect among the Europeans and Asians, because such a discussion raises the possibility that the causes are unique to these peoples, or similarly situated peoples. This might open the door to a politically incorrect consideration of the possibility of racial differences.

Thus, the authors are reduced to pointing to Australian evidence showing "the use of red ochre, the practice of cremation, and the creation of paintings, engravings, and necklaces – all discovered in Australian sites that are 30,000 years old or more" (p. 198), and the fact that Australia was settled by peoples with the implied ability to build water craft that held together for an open water voyage of at least a week.

Thus, having claimed that Australian (Mungo) development occurred about the time that Cro-Magnons appear in Europe, they can ask, "But how can we rationalize the fact that artistic and symbolic ferment was bubbling from Mungo to Lascaux. If we assume the requisitive cognitive transformation was triggered after *Homo sapiens* embarked on its African Exodus, we must explain how it erupted in tribes in different parts of the world. If the Great Change



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occurred as late as about 40,000 years ago, then it must have developed, virtually simultaneously, in people who were living many thousands of miles apart." (p. 198). They resolve this alleged difficulty by assuming that the change had occurred before leaving Africa, an unnecessary theory that raises its own difficulties.

As they ask (pp. 198-199), "On the other hand, if neuronal remodeling was part of our African heritage, why did it take so long to manifest itself? Why, when we look at those populations at Amud, Skhul, Qafzeh, Kebara, and the other Levant sites, can we see precious little differences between the stone tools made by Homo sapiens and our Neanderthal cousins? If we were already bequeathed with our full, final neurological endowment, why did it not manifest itself then? We had begun our exodus, and by inference were kitted out with our complete intellectual baggage. . . So why did our sophistication not show then? Why the delay before it became apparent." These are good rhetorical questions, and ones I have tried to answer myself (Miller 1997).[6]

The evidence (as the authors note) is quite strong that the early ancestors of Europeans displayed less intelligence on exit from Africa, than they displayed upon their appearance in Europe as Cro-Magnons. The primary evidence comes from the Levant, where the remains of early moderns are associated with Mousterian artifacts indistinguishable from those used by Neanderthals.

A fairly obvious possibility is that once in West Eurasia the migrant population evolved somewhat higher IQ's, as a result of selection under a new and challenging, colder climate. This is an area that was probably blocked from substantial gene flows from tropical areas to the south. If the early moderns in West Asia (including the Levant) were the ancestors of all the non-Africans (as migration routes out of Africa imply, see p. 178), some increase in intelligence could have occurred before the populations were spun off that became the Australians. Thus, there is no need to explain how there

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[6] One should never overlook the likelihood that the ancestors of those who migrated to Eurasia would most probably have been resident in the high savannahs of Northeastern Africa for some time, and would have begun the evolutionary separation from populations resident in the tropical rain forests of sub-Saharan African long before they actually left the African continent.



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could have been "simultaneous" evolution in populations as far separated as Australia and Europe, if the ancestors of both were from Western Asia. Even with an Out of Africa date as recent as 100,000 years ago (to use the book's dating), an arrival of humans in Australia, with the technology of perhaps 50,000-60,000 years ago (see p. 178) leaves at least 30,000 years in West Asia for selective pressures to have begun to raise their intelligence.

Here is where the ideological baggage of the book seems to interfere with proposing the obvious, namely that further intellectual evolution occurred *after* leaving Africa. The political problem is that if one allows this possibility, one must then ask what caused this evolution (movement into new enviroment, with a more challenging climate, is the obvious possibility, so long as further gene flow from tropical areas were blocked). One then must assume that those left in Africa did not experience the same novel challenges, and being alread well adapted to the problem of survival under the prevailing conditions did not experience the same selection in favor of higher intelligence. If the ancestors of European Cro-Magnons were shaped by the novel evolutionary forces they experienced in the 5000 or more generations after leaving Africa, other migrant groups could have been shaped by the evolutionary forces they experienced. There is then a possibility that the migrant groups, dispersing over such a wide area as Eurasia, would come to differ in different parts of Eurasia, and more especially come to differ significantly from those who remained in Africa. To admit this is politically unacceptable, and it is interesting to watch the authors dance around it.

The obvious alternative is to look at the evidence, note that the earliest moderns Out of Africa used primitive technology (Mousterian), and conclude that significant intellectual evolution occurred outside Africa, even before the emergence of Cro-Magnon man. I have made this argument elsewhere (Miller 1997).[7]

The available trees tracing human descent show that after leaving Africa one branch diverged to evolve into the Australian and

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[7] Admittedly, there is the evidence from Katanda in Zaire dated to 90,000 years ago of bone harpoons, knives, and huts (p. 181) but because of the location, these were probably not made by the ancestors of the moderns who left Africa.



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Southeast Asian peoples (see the chart based on the work of Cavalli-Sforza and colleagues on p. 137 of *African Exodus*). It is plausible that the people that populated Australasia and neigbouring Southeast Asian territories (prior, in the latter case, to admixture with incoming Mongoloids from East Asia) are those who moved from West Asia into the tropical regions of Southeast Asia (and then to Australia). These were subject to less selective pressure for intelligence there than the peoples who evolved into Caucasoids and Mongoloids in the colder, more northerly latitudes.

One should not take too seriously speculation about what happened after humans left Africa because the actual number of human remains and artifacts are few. The reasonably hard data points are the primitive Mousterians tools found with the early humans in the Levant (about 100,000 years ago), and the evidence for a much more sophisticated Cro-Magnon material culture appearing about 35,000 years ago.

There is one major implication of the recent research showing the Neanderthals were not the ancestors of modern Europeans. If we are to seek the ancestors of modern Europeans (and their American descendants), what is needed is not further European excavations, but excavation in the virtually unexplored areas of the West Asia and Africa. In the colder periods, when Neanderthals were to be found in the Levant, we must ask where were the ancestors of the the Cro-Magnons and modern Europeans? I would suggest they had retreated to a warmer area in West Asia, where they were isolated from gene flows from the tropics and remained under selective pressures from the relative cold. Suitable digs in West Asian rock shelters stand a chance of finding remains from say 50,000 or so years ago that are plausibly ancestral to Cro-Magnons and Caucasoid populations. Such findings could provide important clues as to what selective forces helped shape the ancestors of the Caucasoid peoples (and possibly of other non-African races). Of course, further work in Africa may cast light on the even earlier period before humans left there. Africa is a vast continent and its geography suggests that it was the Northeast quadrant that shaped the ancestors of those that left Africa.

For reasons discussed elsewhere (Miller 1997), the hypothesis that a subsequent migratory wave of modern peoples Out of Africa displaced the Neanderthals in the Levant and moved rapidly into



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Europe as fully-fledged Cro-Magnons is implausible. It would imply that the Europeans were more closely related to the Africans than to the Australians and Asians, and this is not what is found.

In conclusion, *African Exodus* does a reasonable job of explaining the Out of Africa hypothesis and why it should be accepted. However, it errs badly when it tries to argue that because we all are descended from hominids domiciled in Africa, that therefore there can be no meaningful biological differences between the peoples of the world. It is implausible that no further evolutionary changes took place in the upwards of a hundred millenia after the first humans left Africa, as humans stuggled to survive and reproduce in the widely disparate environments they came to occupy, ranging in climate from tropical to Arctic.

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In an unusual move the article by Professor Furedy which appeared in *The Mankind Quarterly*, 1997, Vol. 37 no. 4, Summer issue, is here reprinted in full, incorporating corrections which did not reach the typesetter before the journal went to press. The decision to reprint was made in view of the importance of the paper. In addition, the author's own preferred abstract and suggested Key Words have been incorporated in the corrected version which appears below.

# The Decline of the *Eppur si muove* Spirit in North American Science: Professional Organizations and PC Pressures

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Galileo's sub-voce and probably apocryphal denial of the geocentric position represents the value of disinterestedness – that what governs enquiry is the search for truth. This attitude has come to govern the physical sciences, and, to a certain extent in the 20th century, the biological ones. However, the social sciences and humanities are more obviously influenced by politically correct (PC) pressures. Still, the PC-derived culture of comfort has affected even scientific organizations in North America, and this note presents four relatively simple stories that illustrate this abandonment of the *Eppur si muove* spirit. The stories, which recount how organizations have handled controversial and (to some people) uncomfortable issues, suggest that it is not only the humanities and social scientists that have to worry about the PC virus; physical and biological scientists and their organizations are not immune.

KEYWORDS: Disinterestedness, Political Correctness, Scientific Organizations, Culture of Comfort

Following his recantation of the heliocentric theory, Galileo was said to have uttered, sotto voce, the words "*Eppur si muove*" ("And yet it moves"). The story is probably apocryphal, but the moral behind it is important. The significance of the story lies not in the correctness of Galileo's theory as a whole – the sun may be the center

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of the solar system, and, in that sense "stand still", but it is far from being the center of the universe, and, like the earth, moves within it – rather, the significance of Galileo's remark is the attitude it has come to represent.

The remark implies that no matter what an authority may say, even if that authority has power over life and death, it has no power over truth. In the context of current North American political correctness (PC) within academia, if the presentation of a particular scientific view produces "discomfort" or strong feelings of being "offended" in many people, there is a trend for even scientific organizations to play a church-like role in trying to exercise power over truth. Those who are not governed by such tenets would insist that considerations like discomfort or offensiveness are not criteria for the validity of the view being proposed. Moreover, even the successful suppression of discomforting views does not have any impact on their actual truth or falsity.

Galileo's remark represents the value of disinterestedness[2] – that what governs enquiry is the search for truth. The disinterested approach was first systematically practiced among the pre-Socratics, a group of Ionian philosophers (of whom Thales was the first) who discussed the question of what is common to all things. The "Greek way of thinking about the world"[3] is in many ways epitomized by the life and death of Socrates. He was devoted to the search for truth even if this brought him into conflict with prevailing social values. Socrates' opposition to the PC of his era included his noting that "although the many can kill us, that is no reason to value their opinions over the knowledge of the wise". This attitude should apply throughout enquiry, ranging from the humanities, through the social sciences, to the "harder" biological and physical sciences. It was to astronomy that Galileo applied his epistemological values, and soon after his death, the approach gained widespread acceptance for all the physical sciences. Similarly, soon after Darwin's death, support grew

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[2] For a more detailed account of disinterestedness, see John J. Furedy, "Daniel Berlyne and disinterested criticism: Inter- and intradisciplinary discourse", In G. Cupchik & J. Laszlo (Eds), *Emerging visions of the aesthetic process: Psychology, semiology, and philosophy*. Cambridge Univ. Press, Cambridge, U.K., 1992.

[3] John Burnet, *Early Greek philosophy*. London: Adam & Charles Black, 1930, p. v.



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for the disinterested approach in the biological sciences, although that support was far from universal, especially outside the academy. With respect to the humanities and possibly to the social sciences as well, however, a proportion of academic thinkers argue that "value free" enquiry cannot be attained, and so disinterested research is impossible.[4] Out of the arguments about the nature of values in scholarly analysis in the humanities, an emphatic relativism has emerged. Where this relativism has been accepted, scholarship has become markedly susceptible to PC pressures.

While scholars in the harder sciences mostly believe that they have avoided the pitfalls of relativism in its various forms, they should not be complacent. The harder sciences are not immune to pressures that undermine the *Eppur si muove* spirit. The biological sciences are especially vulnerable, as many of their propositions impinge on the "comfort" of contemporary society, just as the propositions of astronomy impinged on the "comfort" of many of Galileo's contemporaries. The "culture of comfort" that has so affected North American institutions of higher education[5] has certainly begun to influence, if not biological science, then some biological scientists.

Some of the most compelling evidence for this trend can be found in some recent actions of the formal organizations of these scientists in North America. While the public mission statements of the

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[4] For this constructionist view see, for example, the assertion of an eminent social psychologist that "we do not discover facts; we invent them" (Scarr, 1985, p. 499). "Constructing psychology: Making facts and fables for our times". *American Psychologist*, 40. 400-512.

[5] For a more detailed analysis, see John J. Furedy, "Academic freedom versus the velvet totalitarian culture of comfort on current Canadian campuses: Some fundamental terms and distinctions". *Interchange*, in press, 1996. The culture of comfort, moreover, encompasses both intellectual and emotional discomfort, and many put these considerations ahead of disinterested enquiry. As a Canadian historian concerned with academic freedom has noted, "Many people, not a few of them teaching in universities, find a threat in the unhindered discussion of ideas or the implications of certain lines of research. Such people wish to control or eliminate sources of intellectual or emotional discomfort, or to end the 'waste' of money that is implied by the term 'idle curiosity', [and wishing to] restrict academic freedom, will warn that 'excessive' freedom turns into licence, with harmful consequences for the university and the public." (Michael Horn, "Academic freedom and tenure. Part one: an academic good, a societal benefit," *Society for Academic Freedom and Scholarship Newsletter*, #15, Dec. 1996, p.4.



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organizations are usually consistent with their scientific orientation, the ways in which the organizations behave when it comes to dealing with "uncomfortable" topics that are raised by individual members in their organizations frequently reveals a bowing to PC influences.

The purpose of this note is to focus on the reactions of these North American scientific organizations to ideological and social pressures. I suggest that, in some cases, the organizational reaction to "uncomfortable" topics is PC-influenced, resulting in an abandonment of the *Eppur si muove* spirit. In this paper, I comment on four recent incidents that I believe illustrate this abandonment. All the stories are quite simple to understand from the perspective of disinterestedness, although understanding the details of the relevant scholarly controversies requires expertise in a number of disciplines.[6]

**The Stories**

*Avoidance of "too overt" controversies.* The first tale of abandonment pertains to the American Association for the Advancement of Science (AAAS). It began in late 1995, when a professor of psychology at a major Canadian research-oriented university submitted an abstract for poster presentation to the AAAS annual meeting. This professor is best known for his controversial views on behavioral group race differences in such aspects as intelligence, criminality, and sexual promiscuity. Few, if any, submissions for poster presentation are rejected in AAAS, the refereeing of poster abstracts being minimal. The professor's abstract indicated that he would present his views on the relation between intelligence and brain size, but did not mention that he would also discuss the relation between intelligence and race. Following a confrontation between the professor and some of his critics at the poster session, the AAAS Program Chair said in a radio interview that in future the AAAS would "more carefully edit" poster abstracts, because the organization did not wish "too overt" controversies to be

[6] Moreover, to actually argue about any of the controversies requires not only expertise in the cognate discipline, but also a detailed familiarity with the recent relevant literature. As neither I nor most readers can satisfy the joint requirements of background and familiarity, I have deliberately not offered any evaluation of the merits of the rival positions in each controversy.



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discussed at its meetings. The program chair also indicated that, in his view, had the professor mentioned race in his abstract, that would have "raised a red flag", and the program committee might well have not allowed the poster to be presented.

These are astounding public policy statements for any modern scientific organization to make. It is clear that the question of whether the professor, who is J. Phillipe Rushton of the University of Western Ontario, is right or wrong is not at issue here.[7] The facts are that: (a) he is a qualified AAAS member, and has actually been a fellow since 1988; b) the race/intelligence issue is of scientific interest; (c) a poster presentation is generally regarded as the lowest-profile format of scientific meetings, it being easiest to get a poster presentation onto the program.

*Preventing "misunderstandings."* The second and third tales concern the activities of an organization specifically devoted to neurosciences, the Society for Neuroscience. At the 1995 meeting, the topic of "What's the Biology of Violent Behavior?" was presented in a session devoted to the discussion of social issues. The chair began on a high note, stating that controversial issues have to be discussed at scientific meetings like this one. He then went on to state that the purpose of such discussion was to prevent "misunderstandings" and "distorted presentations of the issue" that the media engage in, and followed this up by citing recent media coverage of *The Bell Curve* as an example of such "distorted presentations". After this preamble I looked forward to a sharp, if civilized, discussion of the issues.

Following presentations by the panelists, the discussant raised this question: "Should neuroscientists suppress findings that may lead to evil consequences?" When none of the panelists provided an answer to this question, I commented as a member of the audience that the moment that an organization starts down this path, it has transformed itself from a scientific organization to a religious or political pressure

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[7] For a relatively uncommitted psychological perspective on this aspect of the nature/nurture controversy, see John J. Furedy, "Race studies: Contentious but legitimate science – review of J.P. Rushton's *Race, Evolution and Behavior: A Life History Perspective*, and R.J. Herrnstein and C. Murray's *The Bell Curve: Intelligence and Class in American Life*. *Toronto Star*, Book Section, October 12, 1994.



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group. This answer was applauded, but that was the end of it. The panelists politely ignored the question that the discussant had raised, and failed to challenge my contention with counter-arguments.

The third tale concerns the selection of the topic to be discussed within the context of a social issues session, during the 1996 meeting of the same organization. These sessions usually cover topical and controversial issues. There was a plan to discuss "Sex differences" and this was announced in the 1995 meeting. In late 1995, however, the program committee decided to change the topic to that of "funding".

The subject of funding is hardly new, and had been discussed at previous meetings in some form; in 1996 it was to receive special prominence. Funding as a topic is certainly more "comfortable" and less controversial than the sex differences; the discussion usually focuses on how to raise funds for research. Could it be that, possibly unintentionally, the scientists are leaving controversial issues like sex differences for the non-scientific media to cover (and perhaps distort)? This raises the question of how seriously the society is committed to the discussion of genuinely controversial issues at its meetings.

"*For the good of the Association*". In the above three stories, the individuals raising "uncomfortable" topics were not prominent members of the organization in question. My fourth and final story involves a very prominent individual – the organization's past president. The tale, which needs to be told at some length, is about the actions of the Board (also called executive committee) of the Behavior Genetics Association (BGA), following an outgoing presidential dinner address in the summer of 1995 by an eminent psychology professor, Glayde Whitney. The occasion was special for BGA, because it was the organization's 25th anniversary of annual meetings. Whitney's title for that evening's speech was "Twenty-five Years of Behavior Genetics".[8]

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[8] This presidential talk, with a new title ("Ideology and censorship in behavior genetics") and a brief new introduction, has been published by Glayde Whitney in *The Mankind Quarterly*, vol. 15, #4, pp. 327-42, 1995. For brief accounts of the sequel in leading scientific journals, see Declan Butler, "Geneticist quits in protest at 'genes and violence claim', *Nature*, Vol. 378, p. 224, and two reports by Constance Holden in *Science*, 1995,



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In his talk the outgoing president suggested that just as the study of individual differences has advanced in the past several decades, so the study of group differences should be advanced by the examination of genetic and environmental factors (and their interaction) that contribute to those differences. At this level of abstraction it is possible that few of his listeners were offended. What apparently generated great offence is that he chose to use race as an example for group differences, and focused on murder as the phenomenon of interest. Beginning with the fact that, in the U.S., there is a significant and sizeable correlation between race and murder rate, Glayde argued that the correlation could have a genetic as well as an environmental component.[9]

It is an understatement to say that the talk was not received with unalloyed enthusiasm by the audience, some of whom walked out immediately. This in itself is curious, given that the "offended" audience comprised scientists whose area of interest, behavioral genetics, is relevant to the talk's topic, and whose methods of inquiry are quite "hard" in the sense that the analysis of data involves a considerable degree of quantification. However, this reaction was mild compared to the later actions of the BGA's Board.

According to Dr. Whitney, he was "shunned and castigated" at

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entitled, respectively, "Specter at the Feast of Science" (Vol. 269, p. 35) and "Behavior Geneticists shun colleague" (Vol. 270, p. 1125). My account here is based on letters circulated on email by BGA members on a list serve (bganet@lists.Colorado.edu), where every message is signed by an individual, and includes the disclaimer that the message "does not necessarily represent the opinions of the Behavior Genetics Association, its members, or the maintainers of BGAnet". I am grateful to Glayde Whitney for providing me with this material, but of course I accept full responsibility for my interpretation of it.

[9] Commenting on an earlier draft of this paper, another journal's editor has indicated that, in his view, "our readers will probably need to know a bit more of what Glayde Whitney actual said that so infuriated his colleagues", and noted that I have given "considerable space to the aftermath, but very little to the incident that occasioned it". However, in my view the information I have provided (together with the reference above to a published version of Whitney's address) is sufficient. Just exactly what may have caused Dr. Whitney's fellow scientists to get so angry is a matter of psychological speculation. It may also be that Whitney did not convey his material with sufficient "sensitivity" to satisfy so-called experts in sensitivity training. All this, however, is not only speculative. It is also irrelevant for determining whether the BGA's treatment of this controversy constituted an abandonment of the eppur si muove spirit.



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the meeting of the executive committee the following morning, where the changeover of officers was arranged. Documentary evidence of the determination of the executive committee to placate those offended by his address is contained in the communications from the committee to him and the membership. In the first of these missives, the incoming president of the BGA, Dr. James Wilson, emailed an open letter to Dr. Whitney on June 16, 1995. The letter not only asked him to retire from the position of chair of the Awards Committee (a customary post for the outgoing president), but also urged him to resign from the BGA itself. Several other Board members wrote open letters in support of the new president's action, including president-elect Pierre Robertoux, who ended his June 22 letter by stating that "As elect-president, I have presented my apologies to our black brothers after your talk, but it is not enough; I urge you to resign as past president AND as BGA member".

The editor of BGA's official journal, Dr. David Fulker, was at the Board meeting to discuss journal issues. At that meeting he stated categorically that he refused to consider publishing any version of the Presidential address in the journal. In a subsequent June 27 letter to the Board (of which the past president was still a member), Dr. Fulker clearly maintained this anti-publication position, when he wrote that "for my part as editor I have to say that I could not publish such an inflammatory, insulting and divisive piece in our Journal. It does not in any way represent an official position of the Association or even a position that most members would wish to see expressed unless I am very much mistaken". He concluded that "In my view publishing this piece would tear us apart and destroy the Association".

Dr. Fulker's comments could be appropriate were BGA a political or religious organization, but for a scientific organization issues of "divisiveness" created by differing interpretations of data should be secondary. And whether positions are expressed or not should be determined by the relevance of those positions to the



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discipline, rather than by what "most members would wish".[10]

The past president's reaction to these demands was, in a September 5 letter, to accede to his removal as chair of the Awards committee, "in the interests of collegial cooperation," it being clear that the majority of members were unable or unwilling to work with him on activities that usually fall to the past president. However, he refused to resign either from the Board or the BGA. President Wilson's September 3, 1996 letter reiterated the demand that the past president resign from the Board, and now produced a new threat to force this resignation. There were by-laws, he said, that authorized the removal of an officer from the Board "for the good of the Association", and "plans are presently being made, in accordance with the by-laws, to meet and vote on removing you from the board". The letter urged the past president to voluntarily resign from the Board before such a vote, offering to allow him to stay on as a member of the BGA. Somewhat disingenuously (given his previous open letter) Dr. Wilson now wrote that "to my knowledge there is no strong pressure that you should resign from the association – just the Board." The letter concluded with the ultimatum that "we will wait until noon (your time) Wednesday, September 6[11] for your resignation. After that we will continue to arrange for the vote".

The time of the ultimatum passed without surrender, so the Board went ahead. An October 25 letter from President Wilson, on behalf of the Board, informed all BGA members that the Board "plans to hold a special meeting in Washington DC on November 5 to discuss the possibility of removing Mr. [sic] Whitney from the Board", this being "in the best interests of the Association", and that out-of-town Board members were eligible for reimbursement of

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[10] Nor did the Board make any effort to scientifically investigate "what most members would wish to see expressed" at any time during this controversy, although, as Dr. David Lykken's October 30 email (see below in text) noted, the wording of appropriate questions would be an embarrassing task. For example, given that in his talk Dr. Whitney referred to data from FBI Uniform Crime Reports, a question suggested by Dr. Lykken was: "Do you agree that any officer of BGA who publically cites data from the FBI's Uniform Crime Reports shall be deposed and defamed?"

[11] One is reminded of Chamberlain's 1939 September ultimatum, but his deadline to Hitler was for an hour earlier.



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expenses. The letter also informed members that "if we are unable to agree on appropriate action at this meeting", some members of the Board might resign.

Dr. Whitney's open letter of October 29 noted that although, as a current member of the Board, he had been invited to come and "defend myself", and had heard that there was in circulation a "list of charges" or a "statement of my wrongdoing", he had received no such list or statement. He pointed out that even the Inquisition of Rome "informed its victims of its charges", and requested permission for science writers such as Constance Holden to be allowed to come to the meeting as an observer.

From the beginning of this affair, Dr. Whitney had, of course, received support from BGA members and others who still upheld the distinction between a scientific and other sorts of organizations, even if they did not agree with Dr. Whitney's scientific opinions. However, the Board's October 25 special-meeting notification elicited a new wave of criticisms. In an October 30 email, Dr. David Lykken (a distinguished psychological researcher, perhaps best known currently for his twin studies at the University of Minnesota) signed himself as a "former member" of BGA, and wrote of being alarmed to "discover that a scientific society that one has valued for so long has fallen to the governance of a lynch mob."

Eminent author Jared Taylor drew on the Galileo analogy when he stated, in an October 30 email, that to claim that Dr. Whitney was "insensitive" was like arguing that Galileo should have been "sensitive" to the geocentrists rather than trying to prove them wrong. Further, he suggested that so-called scientists who want to "silence or expel their colleagues, or who avert their faces from awkward data, are the intellectual heirs to Stalin, Beria, and Lysenko." He concluded his message with: "tyrannical orthodoxy in the sciences is both contemptible and ridiculous." Yet another email on the same day came from Dr. Csaba Vadasz, who, amongst other things, argued that spending the organization's funds on such a special meeting was inappropriate, because the meeting would indicate that the Board "disregards scientific principles in favor of open censorship, political manipulation, and lack of social responsibility". The Board, in Dr. Vadasz's view, should "apologize to the membership for valuing ill-perceived political correctness more than objective and impartial



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rigorous research in a socially important and sensitive area". On the next day (October 31), a brief email from president Wilson stated that "After telephone conversations, the members of the Executive Committee have, for the good of the Association, decided to postpone indefinitely its planned meeting".

**Envoi**

The tales I have told are simple partly because they involve blatant abandonment of the *Eppur si muove* spirit by these scientific organizations, or, rather, by those members who are in positions of authority in these organizations. I suspect that there are many less blatant instances of abandonment which may be more difficult to document and analyze, but which, nevertheless, are of a cumulative significance in eroding the *Eppur si muove* approach. For example, I wonder whether invitations to participate in high-prestige invited symposia are influenced not only by a scientist's contribution to the discipline, but also by whether that contribution is consistent with prevailing ideology with regard to such issues as the relative contribution that nature and nurture play in determining such behavioral phenomena as intelligence and criminality. If my last tale concerning the treatment of BGA's past president is any guide, my guess would be that such more subtle anti-scholarly influences are at play. However, without systematic research, my opinion is no more than a guess. But such research is necessary, if we are to understand what is going on in current scientific organizations.

Even these simple examples, however, are sufficient to suggest that PC is having a significant effect on North American scientific organizations. This influence, especially when not explicitly examined, can have considerable detrimental influence on enquiry, especially with respect to the teaching of research. If younger researchers in the biological sciences accept implicitly the attitudes represented in my four tales, then the next generation of biological scientists will not even be aware of how far they have strayed from the principle of disinterestedness, a principle that should be central in scientific activity. At least in the social science and humanities, where the influence of PC is often crude, these issues are now being openly discussed. So, in this sense, the dangers from an "unexamined" PC influence in the harder sciences may be even



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greater than that in the social sciences and humanities.

Disinterestedness or the *Eppur si muove* spirit has always had to contend with its enemies, be these the tools of torture of the Inquisition, or the policy of avoiding "too overt" controversies by established organizations that seem to have forgotten the scientific *raison d'etre* for their existence. It is not only the humanists and social scientists that have to worry about the PC virus; physical and biological scientists and their organizations, at least in North America, are not immune.



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# Hans Jurgens Eysenck (1916-1997)
# OBITUARY

Chris Brand
Author of *The g Factor*

KEY WORDS: Behavior Therapy, Psychoanalysis, Psychopathology, Personality, Neuroticism, Extraversion, Heredity, Intelligence.

When Hans Eysenck arrived in Britain from Nazi Germany in 1934 (via France), British psychology was concerned chiefly with the testing of mental abilities. By 1934, Britain's best-known psychological theorist and writer, William McDougall (1871-1938), was in semi-retirement, having failed to persuade a younger generation in America of the importance of instinctive, purposive, genetic and racial factors in human mentality. Thus work on mental tests and factor analysis, as by Charles Spearman and Cyril Burt, at London's University College, seemed the only route towards an objective science of human psychology that still dealt with important issues and had practical relevance. The practical aspiration was that testing would aid selection and counselling in education and the world of work. Safety-first and factorial sophistry seemed to be the watchwords. Thirty years later, Hans Eysenck, though himself no slouch at factor analysis, had changed that.

While retaining the emphasis on testing – in his case, by personality questionnaires – and biological factors, Eysenck would get the best out of behaviorism both for differential psychology and for psychology as a whole. Accepting the philosophical empiricism of his adoptive country, he would concern himself [some might say 'content himself'] with what could be 'proved' in psychology. In particular, after early research into aesthetics, hypnosis, humor, social attitudes and projective tests, Eysenck would be dismissive of psychoanalytic thinking and reluctant to be drawn into the thankless task of measuring particular human motivations. Instead, after the War, Eysenck was prepared to accept the broad thrust of Russian and American behaviorism, to move psychology towards therapeutic intervention (behavior therapy) and to demand and use experimental



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evidence (even if such evidence had to come chiefly from animals). While accepting the importance of psychometrics and the genetic basis of some of the most important human psychological differences, Eysenck became, with B. F. Skinner, joint leader of a movement that would massively expand the profession of clinical psychology.

Such psychologists would have something to offer patients whose intelligence was limited (including in psychosis) and whose behavioral problems thus required re-conditioning procedures. Suitably, Hans Eysenck's Chair in London University was held at the Maudsley Hospital – London's former 'Bedlam' asylum. His 'progressive', optimistic, best-selling Penguin books of around 1960 made him the star of British psychology; and his pre-eminence among experts in habit-breaking was attested by his editorship of his first academic journal, the quickly prestigious *Behavior Research and Therapy*. Eysenck's most loyal followers (of the London School, deriving its intellectual ancestry from Sir Francis Galton) were undoubtedly those who appreciated his continuing recognition of genetic factors; and this recognition was ever more courageous (especially with regard to racial differences) as crass environmentalism took hold not just of the social sciences but of the media and the soap-opera-viewing public. But a much wider range of psychologists owed Eysenck a debt of gratitude for his stress on conditioning and practical therapy. While retaining the Cartesian concern that psychology be scientific and numerate, Eysenck simultaneously succeeded in maintaining therapeutic optimism. Even his 1960's recommendation of amphetamine as an arousal-increasing cure for excessive extroversion and crime could be taken by psychology's many would-be psycho-social engineers as showing that his heart was in the right place. In the 1980's, at the same time as assisting in tracing IQ differences to biological and genetic roots, he also became keenly involved in high-profile efforts to raise IQ by vitamin and mineral supplementation. When he came to lecture to a capacity audience of 120 in Edinburgh University Psychology Department in December, 1995, his faith in behavior therapy (even for obsessive-compulsive conditions) and in the possibility of psychotherapy preventing lung cancer was unshakable. Though principally a differential psychologist and personality theorist, Eysenck had plenty to offer other psychologists – except perhaps those 'cognitive psychologists' who



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aimed to ignore biology and 'model' human activities on the electronic computer. Eysenck was no therapist himself – though his quiet and kindly manner and attentive ear could easily have qualified him as a Rogerian counsellor; but he had the genius of the true intellectual and strove to do work that was of wide relevance and humane import.

How, then, is Eysenck so often considered a 'controversial' figure? How did his biographer (H. B. Gibson, 1980, *Hans Eysenck: the Man and his Work*, London : Peter Owen) compare him to Freud as a "conquistador" who should best be seen as taking on the establishment of his day – not least within psychology itself? How did Hans Eysenck himself, for his autobiography (1990, London: W. H. Allen), call himself a 'Rebel with a Cause'?

Hans Eysenck grew up in Berlin as the child of an actor father and a film star mother who re-married (to a Jewish film producer) when Hans was nine. Because his mother's work and new liaison required her emigration to France, Eysenck was brought up in Berlin by his maternal grandmother (a one-time opera singer who, having been badly crippled, would herself be taken off to die in a Nazi concentration camp). Gibson (op. cit.) records as follows: "Living in his grandmother's flat in Berlin, Hans appears to have had a very free upbringing ... in fairly comfortable circumstances surrounded by literary and cultural influences ... He seems to have been a precocious and self-willed boy, well accustomed to having his own way."

Certainly, Hans was an intelligent and independent-minded schoolchild who once, at eight, bit a schoolteacher (who had insisted that the tone-deaf Hans must sing) and would later contest other teachers' anti-Semitism. In his autobiography, Eysenck records how he stood unmoved through a Hitler rally – only surprised at others' excitement. Being a natural athlete doubtless further boosted his confidence in his own gifts: he became German Junior Tennis Champion.

The young Eysenck had little general respect for adult authority. In 1972 he would observe (in *Psychology is About People*, London: Allen Lane) that "grown-ups were a fraud and a delusion ... their precepts and their actions were miles apart ... it was a toss-up whether they were more vicious or more ridiculous in their behavior." Eysenck certainly had enough going for him to resist his parents'



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determination that he should go on the stage – though he would, of course, become perhaps the greatest academic showman of the field which he finally chose. Eysenck's mastery of self-presentation was once remarked by Arthur Jensen as the two of them toured Australian universities in the 1970s, when they had both become tagged as 'heretics.' Asked by Australian newsies for a 1,000-word statement that could fit a slot on the front page of their evening newspaper, Art Jensen confessed that this kind of thing was not his bag; but he called Eysenck across, and Hans proceeded to deliver what turned out to be exactly 997 words.

Already as a child there were harbingers of Eysenck's later character – individuality, self-possession and flair. He emigrated to Britain and with the outbreak of hostilities sought to join the Royal Air Force, although his German origins barred him from this so that his contribution to the British war effort was as a fire-watcher in 'Dad's Army'. Eysenck early discovered that opposition to Hitler was not all he might have expected it to have been in the England of the mid-1930's. Studying psychology (since his school curriculum had not included enough science and Latin to allow him to read physics), Eysenck soon found himself amongst scholars who were not pro-war. Then, after the War, his psychology colleagues reacted with indignation when he became one of the first (in a Britain that had become very pro-Soviet) to point out that communism had a lot in common with fascism. Eysenck's recognition of 'authoritarianism-of-the-left' (1954, *The Psychology of Politics*, London: Routledge, New York: Praeger) revealed to psychologists a less cozy side to an academic whose Penguin books were becoming popular with a public which had learned to espouse liberalism and humanitarianism. Eysenck's later willingness to take on feminism (e.g. in Ihsan Al-Issa's *Gender and Psychopathology*, 1982) confirmed his beyond-the-pale status among contemporary social psychologists.

Nevertheless, culminating in the landmark works *The Biological Basis of Personality* and *Personality Structure and Measurement* (the latter co-authored with his wife, Sybil) the 1960's saw Eysenck established as a towering figure in British psychology. Though some found his hereditarianism about crime too daring, Eysenck's position was broadly respected and his sallies against psychoanalysis and the then-unproved causal link between smoking and cancer were enjoyed.



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His failure to clinch his linkage of crime to extrIversion made Eysenck look afresh at other options. With the spur provided by Arthur Jensen's troubles of 1969, Eysenck took up the standard for the 'general intelligence' factor ('g') where Burt had left it. The year 1967 produced what was perhaps Eysenck's single most influential contribution – the link between general intelligence and mental speed. Eysenck went on to support Jensen's genetic linkage of IQ to race: with his 1971 book *Race, Intelligence and Education* (1971, London: Temple Smith) – for which he would be physically assaulted in London by "progressive intellectuals" who had bussed down from Birmingham University. Then, around 1970, Eysenck introduced his claim that psychopathy, psychosis, sexual perversion and genius all had something in common – called Psychoticism ('P'), something measured by questionnaires inviting subjects to declare themselves disillusioned with, cynical about, and suspicious of other people.

Both 'g' and 'P' were to offer much more purchase on crime than had Extraversion. Yet both were to yield continuing controversy. In touching on 'g' differences, Eysenck had trodden on key territory belonging to the then-popular subject of sociology. Eysenck's *Inequality of Man* (1973, London : Temple Smith, San Diego : Knapp) did nothing to dispel suspicions that he saw a wide range of social problems (as well as crime) as arising from people's own mental abilities (or lack of them) – and their genes. Eysenck here was anticipating the row that would eventually burst in 1994 in America over *The Bell Curve*.

The 'P' dimension yielded trouble of a more academic type: there were many criticisms of the scale's skewness (suggesting it tapped interaction effects between other, more basic variables) and about its purely empirical, a-theoretical nature. More seriously, Eysenck's insistence on his 'Gigantic Three' personality dimensions ('N'[euroticism], 'E'[xtraversion] and 'P') put him at loggerheads with virtually the entire world-wide fraternity of psychometrician-psychologists that he had collected around his journal, *Personality and Individual Differences.* Virtually all the academic luminaries of personality testing had come to believe that at least five personality dimensions (their 'Big Five') required recognition – and they did not envisage the 'g' factor as being amongst these. [Despite 'g' being acknowledged by John Carroll (the



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leading American factor-analytic psychologist who was loyal to 'multiple intelligences' – 1993, 'Human Cognitive Abilities'), any mention of IQ and the 'g' factor had by the 1990's become taboo in American universities.]

Around and after his retirement in 1980, Hans Eysenck found further ways of setting the cat among the pigeons. Notably he flirted with (or as friends would say, maintained an open mind about) astrology and parapsychology (even before Camille Paglia and Edinburgh University had 'come out' for these topics) and supported the claims of a little-known German/Yugoslavian doctor that cancer and heart-disease could be prevented by remarkably brief psychotherapy (or 'bibliotherapy', as some dubbed it, in so far as some at-risk subjects were just given a pamphlet!). Yet, 'g' and 'P' provided the foci of Eysenck's main useful work. In particular, Eysenck and co-workers provided increasingly compelling evidence for the link between IQ and mental speed of processing. Even for very simple information: IQ was found to correlate (sometimes substantially) with reaction-times and also with 'brain waves' ('evoked cortical potentials' found in EEG records). As to 'P', Eysenck produced a major review, *Genius* (Cambridge University Press), just two years before his death.

The last year has been an appalling time for what we may call Eysenck's "London School". No serious support has been forthcoming for the message of *The Bell Curve*: to talk of IQ as causal to life successes and abiding racial differences has evidently become increasingly 'controversial' – at least for the mass media. No sooner had Hans Eysenck read my book, *The 'g' Factor*, and generously awarded it high praise, than the book was withdrawn by 'academic publisher' John Wiley. Wiley followed this by rejecting Arthur Jensen's major work on intelligence – which eventually, after rejection by several mainstream US publishers, was scheduled to be published by a small mail-order house offering a service that is only a small improvement on 'vanity' publishing. Loyal Eysenckians and race realists Phil Rushton and Richard Lynn could also achieve publication of their books only by mail-order houses. In November, 1996, leading hereditarian Lee Willerman (University of Texas) died prematurely – leaving Tom Bouchard and Sandra Scarr as the only lively writers emphasizing the role of heredity in the USA. In



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Edinburgh I paid the ultimate academic penalty for my thoughts about race, IQ, eugenics and sex and was sacked, while trying to maintain attention for my de-published book. Now the London School's leading light himself is gone. Yet such disasters will be survived. Despite everything, all of us in the London School have been prepared by the victimization that Hans Eysenck had to endure as he spoke words of truth in defence of Arthur Jensen and other "hereditarian" scholars; and all of us are determined on the same 'bloody but unbowed' attitude which William McDougall commended and which came naturally, apparently from his earliest years, to Hans Eysenck. Truth will prevail in the end, any Westerner must believe. It is just a question of time – as for Galileo (now forgiven by the Pope) and for Darwin (eventually buried in Westminster Abbey, thanks to Galton's initiative). Hans Eysenck will eventually stand as the psychologist who kept the London School together and advanced it massively, even if, *qua* 'rebel', there was never long to wait before the next assault on unmerited authority and ideologically motivated ignorance.

## CHRONOLOGY

1916 Born March 4 to Eduard Eysenck (actor) and Ruth Eysenck (née Werner; stage name Helga Molander). The marriage soon collapsed; Hans was entrusted to Ruth's mother (Frau Werner, once an opera singer); and Ruth married Max Glass (film producer and writer). As the Nazi influence grew, Ruth and Max, who were Jewish, were forced to leave Germany for France. Frau Werner, who had been badly crippled, would eventually die in a concentration camp. After the fall of France, Max's fortune was exhausted in efforts to obtain Ruth's release from internment; but she eventually joined him in South America, a new fortune was built, and the couple returned to Paris.

1926 Holiday at Newhaven. (Eysenck had relatives in Britain but did not discover this till much later.)

1930 Attended boarding school in the Isle of Wight. (In Germany, Eysenck refused to join the Hitler Youth and became a member of the Social Democratic Party.)

1933 Study leave in Exeter (studying literature and history).



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(One of Eysenck's tasks was to bring out of Germany as much of the family fortune as possible – converted into antique postage stamps etc. Another task was the care of his grandmother.)

1934 Leaves Germany for Dijon and London. Enrolled at Pitman's College to prepare for London Matriculation examination (in French, Mathematics, English and Physics).

1935 Enrolled to read for Psychology Honors at University College London. [The Psychology Department had been created with the appointment of William McDougall as Reader in Experimental Psychology in 1903 and was subsequently developed by Charles Spearman and Cyril Burt.]

1937 Appeared on television (Nov. 4) in 'Experiments in Science', a program organized by Cyril Burt.

1938 B.A., University of London. Marriage to graduate student Dr. Margaret D. Davies (with whom Eysenck published one article, on aging, in 1946). (Eysenck's son Michael, today Professor of Psychology at London's Royal Holloway and Bedford New College, was born to this marriage in 1944. At forty, Michael would apply for the Chair of Psychology at Edinburgh and be turned down in a major misjudgment by the University: his *Handbook of Cognitive Psychology* was soon a best-seller, and his book with his father, *Personality and Individual Differences* was favorably reviewed in *Nature* – a rare distinction for psychological authors.)

1939 'Review of L. L. Thurstone's (1938) *Primary Mental Abilities* (*Brit. J. Educational Psychol.*); ["... Eysenck applied Burt's method of factor analysis to Thurstone's correlation matrix of 56 diverse tests and showed the existence of a large general factor, equivalent to Spearman's g, on which all of the tests were loaded and which accounted for more of the total variance than any of the primary factors identified by Thurstone's method of multiple factor analysis which rotated the several primary factors to simple structure, a procedure that, of mathematical necessity, obliterated the general factor, creating the false impression that a general factor did not exist in the matrix." (A. R. Jensen, 1997, in H. Nyborg, *The Scientific Study of Human Nature*) To take on America's leading psychometrician while himself barely 23 was a remarkable coup – though Eysenck would obviously have had Burt's help. As Jensen records (*op. cit.*), Burt said Eysenck was his "most brilliant and industrious student."



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1940 Ph. D., University of London (on the psychology of aesthetics).

1941 'An experimental study of the improvement of mental and physical functions in the hypnotic state' (*Brit. J. Medical Psychol.*). [This research followed up results of William McDougall, summarized in McDougall's (1926) *Outline of Abnormal Psychology*. Eysenck lectured extensively on hypnosis during the war years and Gibson records he was then "the foremost authority on hypnosis in Britain." In his sixties, Eysenck would say that, if he were starting out in psychology again, with a free hand, he would work primarily on hypnosis.]

1942 Appointed Research Officer at Mill Hill Emergency Hospital [where the Maudsley Hospital had been evacuated].

1944 'General and social attitudes' (*J. Social Psychol.*); 'Types of personality: a factorial study of 700 neurotics' (*J. Mental Science*).

1945 'Graphological analysis and psychiatry: an experimental study.' (*Brit. J. Psychiatry.*); 'Primary and secondary suggestibility: an experimental and statistical study' (with D. Furneaux; *J. Experimental Psychol.*).

1947 *Dimensions of Personality*; granted British citizenship.

1948 'Neuroticism and handwriting' (*J. abnormal & social Psychol.*).

1949 'Training in clinical psychology: an English point of view' (*American Psychologist*) [At this point, Eysenck still took the conventional British view that psychologists should not themselves give therapy.] Visit to USA to familiarize himself with clinical psychology there. [He was impressed by the parapsychology laboratory at Duke University set up by Joseph and Louisa Rhine, who had themselves first worked under William McDougall.]

1950 Appointed Reader in Sub-Department of Psychology, Institute of Psychiatry; 'Criterion analysis – an application of the hypothetico-deductive method to factor analysis' (*Psychol. Review*); 'Schizothymia-cyclothymia as a dimension of personality: 1' (*J. Personality*)

1951 'Primary social attitudes as related to social class and political attitudes' (*Brit. J. Sociology*); 'The inheritance of neuroticism' (with D. B. Prell; *J. Mental Science*).



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1952 'The Scientific Basis of Personality'; 'The effects of psychotherapy' (*J. Consulting Psychol.*); 'Schizothymia-cyclothymia as a dimension of personality: 2' (*J. Personality*); Oxford lecture to British Psychological Society (Eysenck expressed his doubt as to the efficacy of psychotherapy and a professor of psychiatry ran down the aisle shouting 'Traitor!').

1953 Married Sybil Rostal (daughter of Viennese violinist).

1953 *The Structure of Human Personality; Uses and Abuses of Psychology* [Penguin] (critical of psychoanalysis); 'The logical basis of factor analysis' (*American Psychologist*).

1954 *The Psychology of Politics*.

1955 Appointed Professor of Psychology, Institute of Psychiatry, University of London – in charge of the all-postgraduate Psychology Department; 'Cortical inhibition, figural after-effect, and the theory of personality' (*J. Abnormal & Social Psychol.*)

1956 *Sense and Nonsense in Psychology* [Penguin]; *The Causes and Cures of Neurosis* (with S. J. Rachman); 'The inheritance of extraversion-introversion' (*Acta Psychologica*); 'The psychology of politics and the personality similarities between fascists and communists' (*Psychol. Bulletin*); 'Maudsley Medical Questionnaire' (*Revista di Psicologia*).

1957 'The Dynamics of Anxiety and Hysteria'; 'Drugs and personality' (with H. C. Holland and D. S. Trouton; *J. Mental Science*); 'Perceptual Processes of Mental Illness' (With G. W. Granger and J. C. Brengelman).

1958 Lecture to Royal Medico-Psychological Association (critical of psychiatry and urging separate development for clinical psychology); 'A short questionnaire for the measurement of two dimensions of personality' (*J. Applied Psychol.*).

1959 'Review of the Rorschach Test' (in O. K. Buros, *5th Mental Measurement Yearbook*); 'The Maudsley Personality Inventory'; 'Learning theory and behavior therapy' (*J. Mental Science*).

1960 *The Structure of Human Personality* (1st edition); *Handbook of Abnormal Psychology* (ed.); 'Behavior Therapy and the Neuroses'; 'Conditioning and personality' (*Brit. J. Psychol.*).

1962 'The position of hysterics and dysthymics in a two-dimensional framework of personality description' (with G.



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Claridge; *J. abnormal & social Psychol.*); *Know Your Own IQ* (giving annoyance to many in the testing movement).

1963 'Experiments with Drugs'; 'The biological basis of personality' (*Nature*); 'On the dual nature of extraversion' (with Sybil Eysenck; *Brit. J. social & clinical Psychol.*).

1964 D. Sc., University of London; 'The biological basis of criminal behavior' (*Nature*); *Crime and Personality* (1st edition); 'Manual of the Eysenck Personality Inventory' (with Sybil Eysenck); *Experiments in Behavior Therapy* (ed.); Personality and the measurement of intelligence' (with O. White); 'The effects of psychotherapy reconsidered' (*Acta Psychologica*); 'Biology and science' (*New Society*). Addressed a large audience at Oxford University Psychology Society.

1965 'Smoking, Health and Personality'; 'Experiments in Motivation' (with P. L. Broadhurst); *Fact and Fiction in Psychology* [Penguin]; 'Extraversion and the acquisition of eyeblink and GSR conditioned responses' (*Psychol. Bull.*).

1966 'The Effects of Psychotherapy'; 'Check Your Own IQ'.

1967 'The Biological Basis of Personality'; 'Intellectual assessment: a theoretical and experimental approach' (*Brit. J. Educational Psychol.*); 'On the unitary nature of extraversion' (with Sybil Eysenck; *Acta Psychologica*); 'Salivary response to lemon juice as a measure of introversion' (with Sybil Eysenck; *Perceptual & Motor Skills*); 'Personality patterns in various groups' (*Occupational Psychology*); 'Personality and extrasensory perception' (*J. Society for Psychical Research*).

1968 'A factorial study of psychoticism as a dimension of personality' (with Sybil Eysenck; *Multivariate Behav. Research*); 'A theory of the incubation of anxiety/fear response' (*Behavior Research & Therapy*).

1969 *Personality Structure and Measurement* (with Sybil Eysenck)

1970 *The Structure of Human Personality* (3rd edition); 'Explanation and the concept of personality' (In R. Borger & F. Cioffi, *Explanation in the Behavioral Sciences*); 'Autobiographical sketch' (In G. Lindzey, *A History of Psychology in Autobiography*).

1971 *Race, Intelligence and Education* (defending the views of Arthur Jensen, the ongoing victim of a witch-hunt organized in the



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USA by the Party for Workers' Power) (Many of Professor Jensen's own colleagues had tossed him to the wolves.); 'Social attitudes and class' (*Brit. J. soc. & clin. Psychol.*).

1972 'Primaries or second-order factors: a critical consideration of Cattell's 16PF battery' (*Brit. J. soc. & clin. Psychol.*); *Psychology is About People* [dedicated 'To Sybil, my love']; 'The personality and attitudes of working class British communists and fascists' [being Thelma Coulter's thesis work, 1953 – she had died in a car crash, so her Ph.D. under Eysenck had remained unpublished] (*J. Social Psychol.*); 'Conditioning, introversion-extraversion and the strength of the nervous system' (with A. Levey; in V. D. Neblitsyn & J. A. Gray, *Biological Basis of Individual Behavior*).

1973 *The Inequality of Man*; 'The Experimental Study of Freudian Theories' (ed., with G. D. Wilson); Assaulted by three 'students' while lecturing on IQ and EEG at the London School of Economics. (Eysenck had for some while been fiercely condemned by a group calling themselves 'Progressive Intellectuals' at Birmingham University. They were thought to be identified with the British Communist Party (Marxist-Leninist).) The UK's National Union of Students ordered its branches to prevent lectures on campus by Eysenck. *The ethics of science and the duties of scientists* (British Association for the Advancement of Science).

1974 'Theories of parapsychological phenomena' (*Encyclopaedia Britannica*).

1975 'Manual of the Eysenck Personality Questionnaire' (with Sybil Eysenck); 'The nature of extraversion: a genetical analysis' (with L. Eaves; *J. Personality & Social Psychology*); 'The Future of Psychiatry'; 'Planets, stars and personality' (*New Behavior*); 'Precognition in rats' (*J. Parapsychology*).

1976 'Psychoticism as a Dimension of Personality' (with Sybil Eysenck); 'Sex and Personality'; 'The Measurement of Personality'; 'The learning theory model of neurosis – a new approach' (*Behavior Research & Therapy*); 'Genetic factors in personality development' (In A. R. Kaplan, *Human Behavior Genetics*).

1977 Lecture tour of Australia with Arthur Jensen (attracting student protests, scuffles and smoke bombs); *Crime and Personality*, 3rd edition; 'Personality and factor analysis: a reply to Guilford'



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(*Psychol. Bulletin*); 'Block and psychoticism' (with Sybil Eysenck; *J. abnormal Psychol.*); 'Personality and the classification of adult offenders' (with Sybil Eysenck and J. Rust; *Brit. J. Criminology*); 'You and Neurosis'.

1978 'The Psychological Basis of Ideology' (ed., with G. Wilson); 'Psychopathy, personality and genetics' (with Sybil Eysenck; in R. D. Hare & Daisy Schalling, *Psychopathic Behavior*); 'An empirical study of the relation between astrological factors and personality' (with J. Mayo and O. White; *J. Social Psychology*); 'An exercise in mega-silliness' (*American Psychologist*).

1979 *The Structure of Intelligence*; *The Psychology of Sex* (with G. D. Wilson); 'Memory-scanning, introversion-extraversion and levels of processing' (with M. C. Eysenck; *J. Research in Personality*); 'The conditioning model of neurosis' (*Behavioral & Brain Sciences* – with comments by some thirty authors, including J. A. Gray, J. Wolpe and M. Zuckerman); 'A psychological theory of hysteria' (in A. Roy, *Hysteria*); 'The Causes and Effects of Smoking'; 'Sport and personality' (with D. K. B. Nias and D. N. Cox; *Advances in Behavior Research and Therapy*).

1980 First issue of *Personality & Individual Differences* (edited by Eysenck) featured the report of a correlation of .80 between EEG waves and IQ by Alan and Elaine Hendrickson (who worked in Eysenck 's unit).

1981 *A Model of Personality* (ed.); *The Intelligence Controversy* (with L. Kamin); 'Psychological factors as predictors of marital satisfaction' (with J. A. Wakefield; *Advances in Behavior Research and Therapy*); First *Festschrift for Hans Eysenck* published (ed. R. Lynn) Contributors: D. E. Broadbent, C. R. Brand, P. L. Broadhurst, R. B. Cattell, G. Claridge, D. W. J. Corcoran, L. Eaves, P. A. Young, A. Gale, H. B. Gibson, J. A. Gray, D. C. Kendrick, R. Lynn, D. K. B. Nias, S. J. Rachman, R. D. Savage, G. Wilson.

1982 *A Model of Intelligence* (ed.); 'Astrology: Science or Superstition' (with D. K. B. Nias); 'Explaining the Unexplained: Mysteries of the Paranormal' (with C. Sargent).

1983 'Diagnosis and clinical assessment' (with J. A. Wakefield and A. F. Friedman; *Annual Review of Psychology*); 'Smoking, nicotine and electrocortical activity' (with D. M. Warburton;



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*Psychopharmacology Therapy*); 'Know Your Own Psi-Q' (with C. Sargent); 'Special review of 'The Psychology of the Planets' [Francoise Gauquelin]' (*Person. & Indiv. Diffs.*); 'The roots of creativity: cognitive ability or personality trait?' (*Roeper Review*); 'Parapsychology: status and prospects' (In W. G. Roll, J. Beloff and R. A. White, Research in Parapsychology); 'Methodological errors by critics of astrological claims' (*Astro-Psychological Problems*).

1985 'Personality and Individual Differences: a Natural Science Approach' (with Michael Eysenck).

1986 Publication of *Hans Eysenck: Consensus and Controversy* (eds. Sohan and Celia Modgil; one of a series on 'controversial' psychologists including Lawrence Kohlberg, Noam Chomsky, Arthur Jensen, and B. F. Skinner) (Contributions by H. B. Gibson, N. Martin, P. Costa, A. Jensen, C. Brand, E. Erwin, C. Barbrack, G. Wilson, C. Spielberger, C. Sargent, J. Loehlin, G. Claridge, J. Carlson, J. Ray, P. Kline, A. Lazarus, D. Gilbert, P. Burch, D. Nias. Introduction and Conclusion by Hans Eysenck.); 'Intelligence, reaction time (RT) and a new "odd-man-out" RT paradigm' (*Person. & Indiv. Diffs.*); *Theoretical Foundations of Behavior Therapy* (with Irene Martin).

1987 'Review of 'Scientific Excellence' [D. N. Jackson & J. P. Rushton]' (*Person. & Indiv. Diffs.*); 'Anomalous phenomena and orthodox science' (*Behavioral & Brain Sciences*, pp. 584-5); *Decline and Fall of the Freudian Empire*.

1988 Recipient of American Psychological Association's Distinguished Scientific Award; 'The concept of 'intelligence': useful or useless? (Editorial for *Intelligence*); *The Causes and Cures of Criminality* (with G. H. Gudjonsson).

1989 *Genes, Culture and Personality: An Empirical Approach* (with L. J. Eaves and N. G. Martin; bringing together and providing modern psychogenetic analysis of many years of data from the Maudsley sample of monozygotic and dizygotic twins).

1990 *Decline and Fall of the Freudian Empire* [American edition by Scott-Townsend, Washington]; 'Dimensions of personality: 16, 5 or 3?' (*Person. & Indiv. Diffs.*); 'Biological dimensions of personality' (in L. A. Pervin, *Handbook of Personality Theory and Research*); *Rebel with a Cause: The Autobiography of Hans Eysenck*.



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1991 'Personality, stress and disease: an interactionist perspective' (*Psychological Inquiry*); 'Raising IQ through vitamin and mineral supplementation' (*Person. & Indiv. Diffs.*); 'Smoking, Personality and Stress'; 'Eysenck Personality Scales' (with Sybil Eysenck); 'Science and racism' (Introduction to Roger Pearson: *Race, Intelligence and Bias in Academe*); 'Obituary for Michel Gauquelin' (*The Independent* [London]).

1992 'Four ways five factors are not basic' (*Person. & Indiv. Diffs.*); 'Psychosocial factors, cancer and ischaemic heart disease' (*British Medical Journal*); 'A reply to Costa and McCrae: P, or A and C – the role of theory' (*Person. & Indiv. Diffs.*); 'Primary trait measurement of the 21 components of the P-E-N system' (with P. Barrett, G. Wilson and C. Jackson; *European Journal of Psychological Assessment*).

1993 U.S. Presidential Citation for Scientific Contribution; 'From DNA to social behavior: conditions for a paradigm of personality research' (In J. Hettema & I. J. Deary, *Foundations of Personality*); 'Prediction of cancer and coronary heart disease as a function of method of questionnaire administration' (*Psychol. Reports*); 'The biological basis of intelligence' (In P. A. Vernon, *Biological Approaches to the Study of Intelligence*); 'Creativity and personality: word association, origence and psychoticism' (*Creativity Research Journal*); 'Creativity and personality' (*Psychological Inquiry*); *Explaining the Unexplained: Mysteries of the Paranormal* (with C. L. Sargent; 2nd edn.).

1994 US William James Fellow Award; 'A biological theory of intelligence' (In D. K. Detterman, *Current Topics in Human Intelligence*); 'Media-analysis and its problems' (*British Medical Journal*).

1995 *Genius: A Natural History of Creativity*.

1996 U.S. Centennial Award for Distinguished Contributions to Clinical Psychology; 'Special review of 'Hormones, Sex and Society [Helmuth Nyborg]' (*Person. & Indiv. Diffs.*); 'Special review of 'The 'g' Factor' [Chris Brand]' (*Person. & Indiv. Diffs.*).

1997 Second *Festschrift for Hans Eysenck* published (ed. Helmuth Nyborg; Contributors: M. Zuckerman, C. R. Brand, J. A. Gray et al., J. Strelau, B. Zawadzki, D. K. B. Nias et al., Sybil Eysenck, A. Raine,



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G. H. Gudjonsson, G. D. Wilson, W. Revelle, A. R. Jensen, P. A. Vernon, R. Lynn, I. J. Deary, N. Brody, H. B. Gibson, Irene Martin, G. Claridge, R. M. Stelmack, J. P. Rushton, H. Nyborg, A. Furnham, S. Ertel). Died at home, September 4. In press 'Personality and the biosocial model of antisocial and criminal behavior' (In A. Raine et al., *Biosocial Bases of Violence*).

If our heredity plays a large role in determining personality, as Eysenck has argued, then Hans Eysenck may have been born to be the center of attention whatever field he chose to enter. Eysenck was born in Germany in 1916 into a family of celebrities. His father, Eduard Eysenck, was an accomplished actor and singer, something of a matinee idol in Europe. His mother, whose stage name was Helga Molander, was also a silent film star. They planned a glamorous career in the entertainment field for Hans, who at age 8 had a small role in a motion picture. However, like many Hollywood marriages today, Eysenck's parents divorced when he was young (only later to marry other show business people).

Although he never pursued the career in show business his parents desired, this does not mean he has avoided the public's eye. In addition to his widely respected work and high level of productivity (more than 50 books), Eysenck often appeared to seek out and dive right into some of the biggest controversies in psychology. In 1952 he published a paper challenging the effectiveness of psychotherapy. He was especially critical of psychoanalysis, pointing out that empirical evidence at the time showed psychoanalysis to be no better than receiving no treatment at all. Because of his early acknowledgment that individual differences in intelligence are largely inherited, Eysenck was often grouped with those who argued that blacks are inherently less intelligent than whites. In 1980 he published a book arguing that the case for cigarettes as a cause of health problems was not strong.

The lifelong combative style caused one biographer to call Eysenck the "controversialist in the intellectual world" (Gibson, 1981, p. 253). Eysenck would no doubt enjoy this title. "From the days of opposition to Nazism in my early youth, through my stand against Freudianism and projective techniques, to my advocacy of behavior therapy and genetic studies, to more recent issues, I have usually been against the establishment and in favor of the rebels," he wrote.



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"[But] I prefer to think that on these issues the majority were wrong, and I was right" (1982, p. 298).

The fullest biography for Hans is still that by one of Hans' students, H. B. Gibson, 1981, *Hans Eysenck: the Man and His Work*, London, Peter Owen. When in doubt as to details in the above, I have preferred Gibson's version to others. Also, I would like to say that I find Gibson's (Chapter 9) analogy of Eysenck with Freud, both of them "conquistadors", thoughtful and indeed compelling.

Hans wrote his own autobiography and provided an autobiographical sketch in G. Lindzey, 1979, *A History of Psychology in Autobiography*, San Francisco, Freeman. An account of the problems of IQ-psychologists Eysenck, Jensen, Rushton, Lynn and Brand with the censorious forces of social environmentalism, latter-day Marxism and 'political correctness' is given by Roger Pearson (1991, 1997) in his *Race, Intelligence and Bias in Academe* (Washington, D.C.: Scott-Townsend), to which Eysenck personally contributed an lengthy introductory chapter entitled "Race and Science".



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*Hans J. Eysenck*
University of London & Maudsley Hospital, London

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# John C. Flanagan and the Development of Psychological Surveys: The U.S. Aviation Psychology Program

Roger Pearson
*Institute for the Study of Man*

An account is given of how John C. Flanagan became a major pioneer of psychological testing field research as a result of survey he conducted into factors affecting family size amongst U.S. Army Air Corps officers. Carried out on the initiative of a small New York non-profit organization, this led to a massive government-sponsored psychological program covering U.S. Army pilots throughout World War II.

KEY WORDS: John C. Flanagan, History of Psychology, Psychological Surveys, Family Size, U.S. Army Air Corps, Dysgenics and War, Pioneer Fund.

John C. Flanagan is a major name in the history of American psychology, having gained wide recognition for his pioneering role in the organization and supervision of extensive field surveys. The best known of these studies involved the psychological testing of U.S. Army pilots during World War II. This was a task he was asked to accomplish following his successful management of a 1938 U.S. Army Air Corps survey which sought to identify the factors responsible for decisions regarding family size made by Army Air Corps officers and their wives. Privately financed, but personally approved by General "Hap" Arnold and confirmed by the War Department, this study opened the door for Flanagan's massive World War II psychological surveys and subsequent research achievements. It was funded by a small New York foundation financed by Colonel Wickliffe Preston Draper, a textile heir who was himself a pilot and a decorated World War I combat hero. While representing the "window of opportunity" that served Flanagan so well in his career as a psychologist, the 1938 study is of intrinsic interest in itself.

## John C. Flanagan

Born in 1906 in Armour, South Dakota, John C. Flanagan was the son of a Baptist minister who had graduated at the University of



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the son of a Baptist minister who had graduated at the University of Minnesota, and of a mother who had graduated from Cornell and had taught school before her marriage. At the age of eighteen, the young Flanagan was an enthusiastic football player who enrolled in the engineering school of the University of Washington, where he played as a quarterback on the college team. However, with only a few credits still required to earn a degree in electrical engineering, Flanagan transferred to the college of education when he found his studies interfering with his football practice. After graduating with a major in physics, he enrolled in graduate school at the University of Washington where he continued to take courses on a part-time basis while launching into a career as a High School science and mathematics teacher and football coach.

Finding himself becoming interested in psychology, Flanagan enrolled in a summer program at Yale University in order to take courses in psychology under E. L. Thorndike, as well as Truman Lee Kelley and other notable scholars. Under their inspiration he decided – in the midst of the Great Depression – to leave his secure teaching position in Seattle to accept a fellowship in psychology at Harvard University where he earned a Ph.D. in mental measurement under the renowned Professor Kelley in 1934.

Flanagan remained at Harvard to work for Walter F. Dearborn, who was collecting data in local schools for the twelve-year Harvard Growth Study. He also worked with Benjamin D. Wood, director of the Cooperative Test Service of the American Council on Education at Columbia University, where he was in charge of the annual achievement test program which collaborated with many leaders in the field of testing and measurement. Here he showed that teacher evaluations in fields such as mathematics and English were too often based on only very general assumptions, and that many teachers failed to evaluate student ability accurately.

As a result, Flanagan's first major academic publication appeared as early as 1935: namely *Factor Analysis in the Study of Personality,* published by Stanford University Press. This was to be followed by a series of articles on the subject of psychological testing, and doubtless contributed to E. L. Thorndike's decision to recommend Flanagan to Dr. Frederick Osborn of the Pioneer Fund as a person qualified to conduct a wide-ranging study designed to determine the factors which



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affected the size of families among young married Army Air Corps officers – and what financial inducements might persuade these young men to produce larger families.

**The Pioneer Fund Army Air Corps Project**

The history of how this came about is of some interest. It was financed by The Pioneer Fund, a New York Foundation set up and funded entirely by Colonel Wickliffe Draper. In the course of his own civil and military career, Draper had come to conclude that the quality of a nation and its culture was primarily dependent on the quality of its population. In addition to general physical fitness, he identified four other human qualities as being of prime significance: high intelligence, courage, altruism and general strength of character. He recognized that these qualities could be found in all populations, but believed that to a significant extent they were genetically determined. He consequently felt concern that nothing should be done to lessen the frequency of genes favoring these qualities in future generations, and conversely that any actions that would serve to perpetuate these qualities inter-generationally would be of enormous benefit to future generations.

To determine how the members of this generation could best be persuaded to transmit a viable biological and intellectual heritage to posterity, Colonel Draper determined to use his own wealth to this end and after consulting a variety of respected academic authorities, determined to form a non-profit organization, The Pioneer Fund Inc. The first meeting of the Pioneer board of Directors was held at 31 Nassau Street, New York, on March 22, 1937, with Dr. Harry Hamilton Laughlin,[1] superintendent of the Eugenics Record Office at Cold Spring Harbor in the chair. Other notable members of the

[1] Dr. Laughlin (1880-1943), a major figure in the U.S. eugenics movement, held a D.Sc. in biology from Princeton. In 1910, he was selected by Charles B. Davenport to succeed the latter as Director of the Eugenics Records Office at Cold Spring Harbor, and was for many years co-editor of *Eugenical News*. Although married, he refrained from having children himself, eventually dying from a genetically-transmitted illness from which he suffered in later years, namely a form of epilepsy.



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board included Dr. Henry Frederick Osborn,[2] and attorney John M. Harlan, who regularly attended Pioneer Board meetings until 1954, resigning only when he was nominated to the U.S. Court of Appeals, prior to his appointment to the U.S. Supreme Court.[3]

After some discussion, this initial meeting of the Pioneer board approved a suggestion made by Colonel Draper that it should investigate the feasibility of funding a program designed to remove financial obstacles to fertility among American citizens of proven worth. Draper's plan had been prompted by his own observation that young flight corps officers, representing a group of individuals who were clearly both intellectually and physically fit, who because of the dangers inherent in their career and their low income which made it difficult for them to educate their children to a level equivalent to their own, were discouraged from passing on their genes to future generations at even normal replacement level. Aviation was still relatively young in the 1930s, but even then it was realized that young pilots represented a select group within society that customarily combined high or above average intelligence, character, and fortitude with general physical fitness. Since young men with those qualities might be expected in general to have married young women of similar

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[2] Frederick Osborn, a nephew and close associate of the renowned paleontologist, environmentalist and eugenicist, Henry Fairfield Osborn, was a graduate of Princeton who obtained his doctorate from New York State University and also studied under Julian Huxley in England. He edited *Heredity and Environment* (1933), co-authored *Dynamics of Population* (1934) jointly with Frank Lorimer, and achieved considerable fame with his book *Preface to Eugenics* (1940), published shortly after he became President of the Pioneer Fund (following Laughlin's resignation due to illness). With American participation in World War II, President Franklin D. Roosevelt appointed Osborn Chairman of the Advisory Committee on Selective Service, and Osborn later rose to the rank of Major General in the U.S. Army. After the war he served as Deputy U.S. Representative on the United Nations Atomic Energy Committee.

[3] John Marshall Harlan was a graduate of Princeton who also studied at Balliol College in England as a Rhodes scholar. Serving as a decorated officer with Bomber Command during World War II, he remained a director of the Pioneer Fund until 1954, when he was nominated to the U.S. Court of Appeals for the Second Circuit, which was followed by his appointment to the U.S. Supreme Court. He had been named after his grandfather, who had served as a U.S. Supreme Court Justice for no less than forty-two years, and this led to occasional confusion – as when a visiting Japanese dignitary, seeing his grandfather's portrait hanging in his chambers, respectfully observed that he had not realized that the office of Supreme Court Justice was hereditary!



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educability, background, and overall qualities – although this needed to be ascertained by objective survey – they were likely to represent a source from which a eugenicist would welcome more children. Colonel Draper's personal experiences as a combatant in World War I, a conflict in which the youthful pilots who engaged in novel but deadly aerial combat high above the trenches suffered the heaviest death rate,[4] led Draper to appreciate the desirability of counter-balancing what was a high death rate and low birth rate among military pilots by means of a eugenically stimulated increase in the number of children born to young married military pilots. The low pay received by a class of youthful officer who faced a high death-risk in wartime, but who belonged to a group which would normally expect their children to enjoy an expensive university education, would clearly discourage reproduction with unavoidable dysgenic consequences. Being a practical man, without children of his own, Colonel Draper determined to contribute personal funds to provide scholarships for the children of serving army pilots for the purpose of encouraging the birth rate among young, married U.S. Army Air Corps officers.

The directors of The Pioneer Board asked Laughlin and Osborn to examine and report on the advisability of offering financial aid for the raising and education of children of officers of the flying corps with the object of encouraging them to have larger families, and at a subsequent meeting of the Pioneer Board, held at the Century Club at 7 West Twenty-third Street on July 6, 1937, Dr. Osborn reported that he and Laughlin agreed that first a psychological survey should be conducted in order to identify the factors which most affected the size of the families of army aviators. They stated that they had discussed the idea of conducting such a survey with both Secretary of War Harry Hines Woodring and officers representing the army aviation service in Washington, D.C., and that both had offered their cooperation. In addition, he reported, an eminent psychologist, Professor E. L. Thorndike, had recommended Dr. John C. Flanagan as a researcher well-fitted to conduct such a study. It was

[4] It has been estimated that during the more severe battles of World War I, the average life of a newly-trained and as yet inexperienced fighter pilot was only fifteen days.



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consequently resolved that ten thousand dollars should be set aside to fund the proposed investigation.

In order to ensure military support for the study, the Directors of the Pioneer Fund made a personal approach to General H. H. "Hap" Arnold, then assistant chief of the Army Air Corps. General Arnold, who later became famous as Commanding General of the U.S. Army Air Forces during World War II, responded enthusiastically and obtained formal approval from the War Department, which also subsequently agreed to Pioneer-financed scholarships designed to assist young army pilots in the education of larger families.

It is of interest to note that at that time there was total of 1,472 officers in the air corps, scattered over forty to fifty stations in the U.S. and its territories. The largest number at any one station was only 154, that being at Randolph Field in Texas. The next largest station, Langley Field in Virginia, had only 122 air corps officers, followed by 109 at Maxwell Field in Alabama. The conduct of the proposed study was made even more difficult by the fact that some five hundred of the officers were scattered in small groups, averaging 10 to 15, attached to smaller army bases around the U.S. and its territories.

Completion of the project was estimated to take about one year, the ultimate goal being to determine not only what factors influenced officers of the air corps to have larger families, but also the effect that various types of financial inducement might have on the birth rate among military aviators. Dr. Flanagan was to be assisted by an advisory committee comprised of a psychologist, a physician, a statistician, a psychiatrist, and an economist was also recommended as a desirable adjunct to the study. Because the wives of officers obviously played a major role in the determination of family size and of the potential excellence of their offspring, Dr. Flanagan recommended that they be included in the survey and was provided with two field assistants to help gather the required data, one of whom was to be female.

John C. Flanagan thus became the first of a long line of distinguished scholars who were to receive financial aid for scholarly research from the small but ably-managed Pioneer Fund. It was his involvement in the Pioneer Fund Army Air Corps study that provided him with the opportunity to make such extensive contributions to the



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development of psychological field work over the following years.

**The Pioneer Fund Army Air Corps Study**

An enquiry form was designed by Flanagan which the U.S. Army agreed to send to all junior flight officers of the Air Corps. This surveyed the educational and psychological background of the officers and their spouses, where married, with particular reference to their attitudes and plans regarding family size. The completed survey and resultant analysis resulted in a report being made to the Pioneer Fund directors in April 1938 which showed that 79 percent of Army Air Corps officers and 23 percent of wives (a high percentage in those days) had at least three years of college education. Flight officers revealed vocational preferences and interests similar to those of engineers. On average the officers had high self-sufficiency ratings, were free from neuroticism, and were by nature self-confident. However, there was an extremely high rate of childless marriages (33 percent throughout the total Army Air Corps) and the mean number of children per marriage was only 1.29! Clearly, this was far below replacement level – and this was a survey taken in peacetime! Colonel Draper's apprehensions that financial and other factors were restricting the size of the families of young Army Air Corps officers had been justifiable.

Another significant factor that emerged from the study confirmed Colonel Draper's impressions once again. One of the major reasons for the low birth rate was that as a result of the low rate of military pay would-be parents in the officer ranks of the aviation corps knew they would not be able to raise more than one, or at best two, children to the level of education they themselves had enjoyed.

The family size deemed by Army Air Corps officers to be ideal averaged out at 3.24 children, but in the conditions affecting the life of an officer in the Air Corps, only a 2.53 average family size seemed to them to be appropriate. Worse, the average officer planned or expected to have only two children, and the prevailing statistics at that time indicated that the average Air Corps officer parent would likely be survived by less than 1.5 children. Indeed, for the Air Corps as a whole, the average family had only 1.29 children.

The factors determining preferred family size were varied. Family background was a major force, and many of the more ambitious



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officers were less interested in raising a large family. Husbands and wives tended to have similar preferences regarding family size. The net reproductive rate of the officers studied was .54, as compared with the rate for the total population of the United States of 1.1, and the rate for U.S. professional groups of .76.

The principal factors responsible for the low reproductive rate of the officers studied were given in the final report as:

1. the expense of raising children to an appropriate level of education;
2. the expense of children to wife in case of the officer's death;
3. the health of the wife;
4. housing;
5. the frequent change of stations;
6. sterility.

Thirty percent of the officers questioned did state that no improvement in their financial status or prospects would affect, or have affected, the size of their family, but that left 70 percent indicating that financial aspects were directly relevant. The report also stated that the cost of each additional child (including $4,000 for college education) was at that time to be estimated at $17,500. Officers of thirty to thirty-four years of age averaged $4,500 income per year, and the possibility of the father's premature death was a major factor limiting family size.

Accordingly, at a meeting at the Century Club in New York on December 15, 1938, at which all directors and officers of the Pioneer Fund were present, it was resolved that a letter be sent to General Arnold outlining a proposal for the Pioneer Fund to offer scholarships and life insurance policies to the children of army aviators, with the object of encouraging larger families among this select group, contingent upon the approval of the army air service. Colonel Draper agreed to set aside an appropriate sum of money, in addition to grants he had already made to the Fund.

General Arnold formally approved the project on December 14, 1938. He warned, however, that the pending threat of war in Europe made a substantial enlargement of the personnel of the Air Corps



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likely, and on April 10, 1939, the legal advisor for the project, John H. Slate, Jr., also warned that the possibility of the U.S. becoming involved in a European war might cause insurance companies to refuse to write policies for Air Corps officers. Despite the situation, the directors decided to proceed with their proposal and to make an award to each child of an air corps officer born in the calendar year 1940 when such child was the fourth (or later) child in the family.

While in past years, on average, only 4.1 children per year had been born to air force officers who already had 3 children, the data from Flanagan's survey indicated that the proposed financial incentive might result in as many as 20 or 30 additional children being born in one year, with a possibility of one in a thousand that this number might reach 50. In consequence, Colonel Draper agreed to guarantee up to two hundred and thirty thousand dollars to the Fund in the event that the project proved more successful than was expected, and an announcement of the scholarship award was published by the military for circulation to potential candidates.

Figures collected at the end of 1940 established that the Pioneer scholarship project had worked. In that year, qualifying Army Air Corps parents added more than double the number of children to their families than would have been statistically expected; and this had occurred despite the chilling effect of the prospect of American involvement in the war in Europe – which, without the security of the proffered annuity would have inclined military pilots to avoid exposing their wives to the risk of becoming widows responsible for supporting four or more children. Annuities were purchased for each of the qualifying children, but as predicted by John Slate, the increasing likelihood of U.S. participation in World War II made insurance companies reluctant to continue to issue policies, and the scheme was not extended to cover 1941. Indeed, with the outbreak of War, Pioneer directors Osborn and Harlan were called to active service, and Donald went to the Pentagon to fill a civilian vacancy equivalent to brigadier general. Colonel Draper himself once again volunteered his services, and left New York for active service in Europe and Asia.

With American participation in World War II, the truly massive and dysgenically selective loss of life among talented young men in



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the military swamped the achievements of Colonel Draper's generous but individually financed eugenic scheme out of all proportion. World War II exerted a drastically dysgenic impact on all the nations which became involved in it – a dysgenic impact of lasting character which can possibly only be equalled by World War I, the Russian Revolution, and events such as the French Revolution and ensuing Napoleonic Wars. Nevertheless, the data collected by Flanagan's extensive and pioneering study inspired the Milbank Foundation to take an interest in further surveys of this kind, to publish a complete monograph on the subject with the aid of a further grant from the Pioneer Fund.[5]

**Subsequent Military Surveys Conducted by Flanagan**

America's participation in World War II actually promoted further psychological research and because of the contacts he had made during his Pioneer Fund study, Flanagan was now commissioned by the army to head a major psychological research project, thus enabling him to display the values of psychological testing on a yet grander scale.

The Pioneer Fund's Army Air Corps study thus laid the basis for Flanagan's future in psychology. As a citation published in the *American Psychologist* (1977, 74), in reference to the APA's decision to award him its Distinguished Professional Contribution Award, states:

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[5] One other project planned by the Fund was halted by Pearl Harbor and America's involvement in the war. In 1941 Dr. Harry Laughlin's health deteriorated to the point that he was obliged to resign from the board of the Pioneer Fund, and Frederick Henry Osborn was elected president in his place. At a meeting on April 21, 1941, Osborn reported that he and Colonel Draper had held discussions with various scientists as to the the possibility of creating an educational museum to inform the public about the impact of evolution, heredity and disparate demographic trends on nations and on the human population of the world generally. A feasibility survey was to be carried out under the supervision of Mr. Calver of the American Museum of Health, with advisory assistance from Mr. Bruno Gebhard of the Cleveland Museum of Health and the support of an impressive scientific advisory committee. But with the outbreak of war each of the Pioneer Fund directors became engaged in full time war duties, and no further enterprises were undertaken by the Pioneer Fund until some time after the conclusion of hostilities.



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> On the basis of a recommendation by Professor E. L. Thorndike, Flanagan was asked by Mr. Frederick Osborn, representing the Pioneer Fund, to do a study of the effect of monetary incentives on the family size of pilots of the U.S. Army Air Corps ... [t]o do the necessary interviewing in the Air Corps in the spring of 1938, he recruited a young flight surgeon, Captain Lloyd E. Griffis ....

It continued (p. 72) as to Flanagan's subsequent wartime activities:

> [Flanagan's] many contributions to professional psychology began when he assembled the staff and directed the program of Aviation Psychology during World War II. Through ingenious test development and careful validation, this program not only made an important contribution to the combat effectiveness of the Air Corps but also demonstrated the potential of this kind of classification testing ... by Project TALENT.

In the spring of 1941, Captain Griffis, having completed his work as Flanagan's assistant on the Pioneer project, was appointed chief of the Research Section, Medical Division, of the Office of the Chief of the U.S. Army Air Corps. General David Grant, the Chief of the Medical Division, agreed that there was a need for a professional psychologist to operate a psychological research agency within that Office, and Dr. Flanagan was granted the rank of major and put in charge of this project. Soon Flanagan was heading a staff of thirty-six psychologists in the Aviation Psychology Program of the Army Air Corps. During the course of the war, this grew to a total of 150 commissioned aviation psychologists supported by more than fourteen hundred additional assistants, all with some training in psychology. The psychologists chosen by Flanagan for the Aviation Psychology Program were selected with extreme care, by consultation with other prominent American psychologists; five of those who worked in this project eventually became presidents of the American Psychological Association, while eleven others became officers or members of the APA board of directors.

The chief functions of the Aviation Psychology Program were to assist in selecting suitable personnel for flight training, to rehabilitate,



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to study ways of improving the training program itself, and to deal with human factors in the design of equipment. As a result of the American Psychological Association the classification type test used for predicting success in flight training rose from a correlation coefficient of .31 in 1941, to .66 in 1943, in the case of 1,000 aviation cadets subjected to aircrew classification tests by the psychological research units.

Before leaving active service, Flanagan assembled a comprehensive series of Army Air Force Aviation Psychology Program Research Reports, which he persuaded the Pentagon to have published by the U.S. Government Printing Office in a total of nineteen volumes. These made a valuable contribution to the science of psychology, and Flanagan himself wrote numerous papers analyzing these over the next few years. On retirement from the Pentagon in 1946, Flanagan, now a Colonel, was able to obtain grants for further research programs from industry, and his total military and civilian research achievements were such that by 1976 he was selected to receive the Distinguished Professional Contribution Award from the American Psychological Association (APA).

The members of the APA committee who selected him for one of the two awards made in that year were S. Don Schultz (chairman), Allan C. Barclay, Kenneth B. Clark, Marie Skodak-Crissey, Robert M. Guion, Bernard Kalinkowitz, Jeanne Phillips, and C. Lee Winder. Previous recipients of the award, which had only been established in 1972, were Carl R. Rogers, David Wechsler, Noble H. Kelley, and George W. Albee. In 1976, for the first time, two awards were made, one of these going to John C. Flanagan.

Flanagan next accepted the position of professor of psychology at the University of Pittsburgh. With the support of the University he established a nonprofit research organization known as the American Institutes for Research (AIR) to improve the understanding of human behavior, where he served for the next thirty years as president or chairman of the board of directors. At AIR he applied his experience as director of the Army Air Corps Psychology Program to the study of industrial, educational, and governmental problems. Amongst these surveys, Project TALENT was conducted on a national level and covered no less than four hundred thousand high school students from 1960 onward. And this was followed by the Palo Alto



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Project, a computer-supported program aimed at correcting defects in American education as identified by Project TALENT.

Thus *American Psychologist* (1993, 718) reported of Flanagan's AIR work:

> The series of studies undertaken to develop critical requirements for jobs resulted in the development, refinement, and expanded use of the important methodological tool that is now known as the *critical incident technique.* It was referred to in the 1979 edition of the *Annual Review of Psychology* as one of the 'most important personnel selection milestones of the past 60 years.' It should be noted that this valuable methodology is not limited to personnel selection but has been found to be useful in a great many areas.

By 1976, when Flanagan was honored with the American Psychological Association Distinguished Professional Contribution Award, the AIR had already carried out more than a thousand research projects in the field of psychology, including testing programs for the selection of pilots for commercial airlines and studies of leadership qualities in both industry and the military.

John C. Flanagan also gave his time to benefit professional associations, serving as president of Divisions 1, 5, 15, and 19 of the American Psychological Association, as president of both the Psychometric Society and the National Council on Measurement of Education. His contributions were recognized by various honors, including the Raymond F. Longacre Award of the Aero Medical Association and the Edward Lee Thorndike Award of the American Psychological Association. However, he never cut his links to the Pioneer Fund, to whom he owed the initial research opportunity which had enabled him to develop his talents so successfully for the benefit of psychology, and continued to consult for Colonel Draper and the Pioneer Fund, without remuneration, whenever needed during the post-war period.



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# Raymond B. Cattell and the Fourth Inquisition

Glayde Whitney
*Florida State University*

Raymond B. Cattell was selected to receive the Gold Medal Award for Life Achievement from the American Psychological Foundation. The award ceremony was canceled at the last minute when threats were made to disrupt the Chicago convention of the APA amid charges that Cattell's work was racist. It took only two political activists to derail the APF. This event is analyzed as an instance of Inquisitional attack on rational thought and inquiry, in the context of modern liberalism with radical egalitarianism.

Keywords: Raymond B. Cattell, Political Correctness, American Psychological Foundation, genetics, eugenics, behavior genetics, race, liberalism, egalitarianism.

The events of August 1997 will assure that the already eminent scientist Raymond B. Cattell will be remembered in history as elevated to the pantheon occupied by such as Roger Bacon, William of Occam, and Galileo Galilei. The infamous events of August and the players will be summarized below, but first a context needs to be established in order to make any sense of the scurrilous attack and the craven response of the American Psychological Association (APA).

## Approaches to Knowledge

The Harvard biologist, historian and philosopher of science Ernst Mayr (1982) has suggested that as human populations evolve from savagery to civilization their approach to knowledge takes one or another of two paths.

One approach leads to modern science, the other to authoritative dogma. The direction toward science, traceable back to the philosophies of ancient Greece, is unique to Western civilization. The much more common direction toward authoritative dogmas is illustrated by the revealed religions that sprang from the Middle East.

The direction toward science traces to the first recorded Western philosopher, Thales of Miletus (c.636-c.546 BC). Thales maintained



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that to gain knowledge and understanding one should start with naturalistic observation, that is, descriptions of events as they exist in the real world. We should then seek natural explanations for natural phenomena. Gods, supernatural beings, and forces or events that were outside the system should not be invoked as explanations for events within the system. A third major position was that it is acceptable, even encouraged, to question existing explanations, to use criticism in order to improve knowledge and theories. These three principles that trace to the beginnings of recorded Western thought capture the essence of modern science; naturalistic observation, natural explanation, and criticism as a beneficial tool to advancing knowledge. Alas, from Thales' time through today his approach has, on a worldwide basis, been a minority position under constant attack.

The road to dogma starts with assertions of knowledge based in authority. Often from a great man or leader come statements, frequently but not always based in revelation. The religious and political aspects of dogmatic systems often become commingled. The revelations leading to dogmas often claim supernatural inspiration, but this is not necessarily the case. Christian theology, Marxian sociology, and Freudian psychoanalytic theory equally well illustrate dogmatic belief systems. The systems with their statements to account for reality become codified into a set of rigid beliefs. Not only is criticism and questioning not encouraged, it is condemned. The less than complete supporter, the doubter, is shunned, outcast, outlawed, a heretic, criminal and evil sinner. Followers will believe on the basis of acceptance of authority ("on faith") and will not deviate from the established dogmas that tend to become ever more rigid. Encounters with the partially understood real world, in all its foibles, always lead to discrepancy between dogma and natural observation of real phenomena.

It is considered necessary to preserve the authoritarian dogma and the power of the authorities in the face of conflicting truths. The Path of Righteousness knows what is good for man and society. Dissenters, free thinkers, or those with new knowledge are viewed as a threat to all that is Good. Sanctions, laws, censorship, need to be imposed and enforced. This is the realm of Inquisitions. In the history of Western civilization there have been four main identifiable inquisitions. It is the fourth that we suffer today.



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**Inquisitions**

*First Inquisition*. The first major inquisition was established in 1233 AD to suppress heresy. The groundwork leading up to the need for this inquisition extends back to the origins of the Christian religion in the west. The few centuries around the time of the fall of the Roman Empire were turbulent. The Roman Emperor Constantine I had his famous vision (312 AD) which led to his establishment of Christianity as the official religion of the Empire. Shortly thereafter the Empire fell; various invading Germanic tribes repeatedly sacked Rome. In the turmoil many of the writings of the ancients, Greek and Roman, were temporarily "lost" to Western civilization. Aristotle, Galen, Thales, were reintroduced only centuries later.

St. Augustine (354 - 430 AD) early systematized Christian doctrine in his monumental *On the Trinity*. He argued against paganism in *The City of God*, and provided what has been called a "classic of Christian mysticism" in his autobiographical *Confessions*. Augustine came to be recognized as the father of theology and over the centuries of the dark ages his approach became official dogma. The essence of Augustinian dogma is that truth must be accepted on faith. And truth resides in the revealed word of God as represented in the Bible and interpreted by the leaders of the Church.

With the "rediscovery" of the learned writings of the Ancients, often acquired from Islam and translated from Arabic back into Latin, problems arose. Here was knowledge, and approaches to knowledge such as Aristotelian deductive logic, not envisioned in the existing dogma. The age of the scholastics was upon the world as scholars tried to incorporate the new knowledge.

Robert Grosseteste (1175 - 1253), Franciscan and first chancellor of Oxford University, studied Aristotle and attempted to integrate the Greek knowledge with Christian dogma. He suggested that there were actually two routes to knowledge, observation with deductive reasoning was one route, while authority (revelation from the written word as interpreted by dogma) was another. In the direction of science, Grosseteste formulated his famous Principle of Falsification: when faced with an apparent conflict between observation and dogma, go with the observation. Experience can falsify the pronouncements of authority.

This won't do at all, hence the Papal Inquisition of 1233. Times



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were dicey for the scholastics. William of Occam (c.1285 - 1349) escaped capture when he fled. In the same year (1264) was published Roger Bacon's *De Computo Naturali* and Thomas Acquinas' *Summa Contra Gentiles*. For his troubles Bacon (c. 1214 - 1294) was imprisoned - 15 years - for heresy. Among the charged crimes was "suggesting novelties". Although it was touch-and-go for Acquinas (1225 -1274), he was eventually sainted and his solutions (*Summa Theologica*) became the new dogma. As had Grosseteste, Acquinas tried to integrate Greek natural philosophy, essentially Aristotle, with Christian dogma. In God's perfect wisdom these two approaches to knowledge will always ultimately agree. However, in our fallibility there will on occasion appear to be a conflict between rational observation (science) and the revealed word (religion). When in doubt, go with revelation. The subsequent hardening of the new theology into dogma set the stage for the third inquisition.

*Second Inquisition*. The second of the major inquisitions was established in 1478 as the Spanish Inquisition. This one was primarily the result of conflicts between competing segments of society. The Spanish monarchy established the inquisition to enforce laws of conversion and to catch false converts. Over the preceding centuries members of the Jewish community had steadily amassed increasing proportions of wealth and power. They, along with Muslims, had been forced to either convert or leave the country. When it was suspected that many of the *conversos* were secretly retaining their Jewish values and culture, the inquisition was established to root them out. A consideration of this second recognized inquisition would lead too far astray for the present essay. MacDonald (1994) provides an in-depth consideration of the Spanish Inquisition from the point of view of the social sciences.

*Third Inquisition*. The third of the main inquisitions was established in 1542 to suppress heresy. As with the first inquisition, a basic problem was that the established authorities would not integrate new knowledge that was discovered after the establishment of their dogmas. Instead the new knowledge was treated as a central threat to all that was good in society. Suppression and censorship was the answer.



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The synthesis of Greek wisdom and Christian theology that was rigidified as dogma after the work of St. Thomas Acquinas included the flat earth with man as the center of the universe. Clearly the Copernican heliocentric theory of the solar system could not be tolerated. Although widely discussed, Copernicus' theory was published only in 1543 when the author was on his deathbed, and then presented only as a speculative thought exercise. It was in 1591 that Giordano Bruno (1548 - 1600) was arrested for a variety of thought crimes, including that he believed the Copernican "theory" to be true.

Andrew White (1896/1965) poignantly wrote:

> But the new truth could not be concealed; it could neither be laughed down nor frowned down. Many minds had received it, but within the hearing of the papacy only one tongue appears to have dared to utter it clearly. This new warrior was that strange mortal, Giordano Bruno. He was hunted from land to land, until at last he turned on his pursuers with fearful invectives. For this he was entrapped at Venice, imprisoned during six years in the dungeons of the Inquisition at Rome, then burned alive, and his ashes scattered to the winds. Still, the truth lived on. (p.125)

It has been pointed out that in the latter decades of the 20th century the fourth inquisition no longer burns its victims, although it has arranged the firing of rather many.

The story of Galileo Galilei (1564 - 1642) is well known to all. Only a decade after the burning of Bruno, Galileo built a telescope. By 1610 he was proclaiming on the basis of new evidence the truth of the Copernican Theory. In essence, "come look through the telescope and see for yourself the evidence for the theory". Arrested by the Inquisition in 1616, he was released only to be re-arrested in 1633. Held under house arrest, the old man was forced under threat of torture to recant.

For the physical sciences the inquisitional suppression and censorship was coming to an end. Indeed, Isaac Newton (1642 - 1727), born in the year of Galileo's death, lived to be knighted and upon death was buried in Westminster Abbey, two of the highest honors from his Church and Country.



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Lagging the physical sciences by a few centuries, the psychological and social sciences are still suffering attempts at suppression and censorship, which characterize the inquisitional approach.

*Fourth Inquisition*. The fourth inquisition was established in the mid-twentieth century to suppress heresy. As with the first and third inquisitions, a main problem has been that the ideologues did not integrate new knowledge with their already established objectives and dogmas. Instead they viewed new discoveries as a direct threat to all that was good and important in society. As with the earlier inquisitions, the fourth attempts to suppress and censor new knowledge that is perceived to be threatening to old dogmas.

Somewhere between Thomas Jefferson and William Jefferson Clinton an influential segment of the intelligentsia lurched far to the ideological and political left. Thomas Jefferson certainly did not confuse rule of law ("all men are created equal") and hereditary reality. In a letter to John Adams, Jefferson wrote, "I agree with you that there is a natural aristocracy among men. The grounds of this are virtue and talents .... For experience proves, that the moral and physical qualities of man, whether good or evil, are transmissible in a certain degree from father to son." (Jefferson, 1813).

In the face of what experience proves, and in open antagonism to much of twentieth century science, a powerful strain of modern liberalism worships radical egalitarianism. Modern liberalism is attempting to enforce Lysenkoism throughout Western civilization. The travesty that is Lysenkoism ruined the science and economy of the Soviet Union. It is well known as an example of the folly of attempting to repeal truth in the service of ideology (Berg, 1988; Medvedev, 1971; Soyfer, 1994). What is less often acknowledged is that the spirit of Lysenkoism is alive and well in the form of modern liberalism's enforcement of radical egalitarianism:

> There and here the guiding theory is identical; it is socialist utopia based on egalitarianism, with what the behavioral scientists call environmental determinism. In 1948 Stalin actually outlawed genetics as being a western bourgeois construction that was incompatible with the truths of Marxist-Leninism. Like outlawing



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> the heliocentric nature of the solar system. Hillary doesn't have quite that political clout, yet.
>
> The theory that Stalin and Hillary share is that all those newborns, wheat plants for Uncle Joe, human babies for Mother Hillary, have identical potentials for growth and development. If some individuals don't do as well as others, it is because of their early experience. This is obviously true - everyone knows that fertilizer is important for wheat plants, and everyone knows that early nutrition and stimulation is important for humans. This is so obviously true that anyone who questions its application to the problems at hand is an idiot, an enemy of the state, and a mean-spirited hate monger. There the eminent scientist who objected, the geneticist Nikolai Vavilov, died of disease and starvation in Gulag. Here eminent scientists that voice objections are subjected to vitriolic ad hominem attacks [And the end of whatever federal research support they may have had]. In addition to individual differences there are those vexatious group differences. There winter wheat and spring wheat did not produce equal crop outcomes. Here it is altogether too obvious that various ethnic/racial groups do not produce equal educational, criminal, or job performance outcomes. Although no one was actually sure of all of the reasons for the differential outcomes, if you did not acquiesce to the environmentalist socialist egalitarian explanation, you were evil, a maverick beyond the pale, beyond the bounds of acceptable discourse. There a hated Morganist-Mendelist, here a contemptuous racist. (Whitney, 1997).

Exactly where and how modern liberalism escaped the bounds of reality is a topic of widespread discussion. The seeds of radical egalitarianism may be contained in the basics of Christianity, with its teaching that all men are equal in the eyes of God (Bork, 1996; Pearson, 1996). Certainly the nineteenth century New England, largely Unitarian, social reformers were influenced not only by their religion, but also by the contemporaneous revolutionaries in Europe.

A major lurch to the left occurred with the bloody French revolution's slogan of "liberty, equality, fraternity". Then there was the 1847 publication of the *Communist Manifesto*, followed by the 1848 wave of riots and revolutions throughout Europe. The 1867 first volume of *Das Kapital* was dedicated to Darwin for the notions of



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evolutionary materialism and progress in the world. However, it is essentially non-biological and like the rest of Marx's writing contains no appreciation of evolutionary biology. In areas pretending to science, as late as 1934 Franz Boas was maintaining that the basis of all serious study was the work of Theodor Waitz. Waitz's major work of 1858 was the pre-Darwinian *On the Unity of the Human Species and the Natural Condition of Man*. This thread was not originally anti-Darwinian; rather it was a-Darwinian or non-Darwinian, an approach to the study of man rooted in biblical creationism with a monogenesist emphasis (Mayr, 1982; Degler, 1991).

Many writers agree that a major wrenching leftward happened with the protest decade of the 1960s. In his autobiographical *Radical Son*, David Horowitz (1997) describes how a group of ideologically committed red-diaper babies, with support and encouragement of the underground Communist Party, engineered much of the radicalism of the 1960s. In *Destructive Generation* Collier and Horowitz (1995) explain that "the utopianism of the Left is a secular religion. However sordid Leftist practice may be, defending Leftist ideals is, for the true believer, tantamount to defending the ideals of humanity itself. To protect the faith is the highest calling of the radical creed. The more the evidence weighs against the belief, the more noble the act of believing becomes" (p. 246).

There is a "readiness to reshape reality to make the world correspond to an idea" (p. 37). There is a "willingness to tinker with the facts to serve a greater truth" (p.37). And so it has obviously been since the 1960s. Over recent decades, as the scientific data accumulate the stridency of the Left intensifies. Driven by ideology and not constrained by the truth, as all else fails they engage in misrepresentation and character assassination.

Raymond B. Cattell described some aspects of the workings of this inquisition which has been snarling at his heels for many decades. In *A New Morality from Science: Beyondism*, Cattell (1972) wrote:

> The danger is not only that politicians and private institutions with axes to grind will find tame or corruptible social scientists to support their positions. The greater danger which recent experiences both here and abroad, e.g., Lysenkoism in Russia, have revealed is that partisans primarily political in interest and intention



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> either accidentally or deliberately infiltrate the ranks of science. In the case of the Lysenko episode, and comparable events in Nazi Germany, the disturbing realization to scientists was that the exile or death of those ejected from their academic positions followed what *seemed* initially to be severe technical criticism by fellow scientists, but was actually politically staged." (p. 38, emphasis in the original).

Robert Bork has commented on a recent high-profile example of "what *seemed* initially to be severe technical criticism by fellow scientists" (Cattell, 1972, p.38). Bork (1996) pointed out that:

> For egalitarians there is always lurking the nightmare that there may be genetic differences between ethnic groups that result in different average levels of performance in different activities. Only that fear can explain the explosive rage with which some commentators received *The Bell Curve* by the late Richard Herrnstein and Charles Murray, which, as a small part of a much larger thesis concluded that there are heritable differences in cognitive ability among the races. Some comments expressed respectful and thoughtful disagreement, some asked for careful reexamination of the data and arguments, but some did little more than shout "Nazi". Herrnstein and Murray are not racists but serious scholars. They may be right or they may not, but the episode indicates the degree to which the ideology of egalitarianism censors expression and thought in sensitive areas. (pp. 267-268).

Many contemporary events amply illustrate the truly inquisitional nature of modern liberalism in the defense of radical egalitarianism. The titles of some papers written by targets of the inquisition are informative, such as "Egalitarian fiction and collective fraud" (Gottfredson, 1994) and "Ideology and censorship in behavior genetics" (Whitney, 1995). While under criminal investigation instigated because of his research, Rushton (1994) wrote "The equalitarian dogma revisited".

It is Christopher Brand, lately of Edinburgh University, UK, who in 1997 suffered the high penalty of being fired for challenging the egalitarian fiction. Having been on the psychology department faculty



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for over twenty years, in 1996 Brand authored a book entitled *The g Factor*. Published in the UK by John Wiley & Sons, one of the largest of the international scholarly houses, the company's promotional literature contained the statement:

> **The nature and measurement of intelligence is a political hot potato. But Brand in this extremely readable, wide-ranging and up-to-date book is not afraid to slaughter the shibboleths of modern 'educationalists'. This short book provides a great deal for thought and debate.**

Brand's book enjoyed brisk sales in the UK for about 6 weeks, and was scheduled for release in the US, when it was suddenly "depublished", actually withdrawn from circulation, seemingly at the command of Wiley's New York executive headquarters. Wiley told the media that the book "makes assertions that we find repellent". Branded a "racist", Christopher Brand was in due course suspended from teaching and administrative duties at Edinburgh University. A "Special Tribunal" was convened, following which Mr. Brand was sacked. At the time of this writing, and in accord with the procedures of classical Lysenkoism, the proceedings of the Special Tribunal remain secret.

The present fourth inquisition is directly analogous with the preceding first and third inquisitions. The agenda and objectives of liberalism were established first before, and then with complete disregard for, Darwin's (1859) *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*. The dogmatic position of modern liberalism with radical egalitarianism was established in a philosophical and political context. The positions were hardened into dogma with no regard for the discoveries of the explorations of the 19th century. Additionally, the genetics and behavior genetics that routinely attacked with religious fervor by the radical egalitarians twentieth century science, not nineteenth century political theology. Marx was writing in the 1840s and 1860s, while Mendel's epoch-setting experiments and theory were not widely appreciated until after 1900.

Unfortunately the radical egalitarianism characteristic of modern liberalism became formalized as a quasi-theological dogma just before



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the discovery of much new knowledge. Just as the first inquisition arose because the existing dogma did not encompass knowledge of Aristotle, and the third inquisition functioned because the dogma was inconsistent with the discoveries of Copernicus and Galileo, so the current fourth inquisition exists in large part because its dogma is inconsistent with the discoveries of Darwin, Galton, and Mendel.

> One must never underestimate what Richard John Neuhaus called 'the profound bigotry and anti-intellectualism and intolerance and illiberality of liberalism.' (Bork, 1996, p. 336).

**The Events of August 1997**

The highest honor bestowed by the American Psychological Association (APA) is the Gold Medal Award for Life Achievement in Psychological Science. As the APA prepared for its 105th annual convention to be held in August, the house organ *American Psychologist* (1997) for August announced the winner of the Gold Medal.

> The American Psychological Foundation (APF) Gold Medal Awards recognize distinguished and enduring records of accomplishment in 4 areas of psychology. The 1997 recipient of the Gold Medal Award for Life Achievement in Psychological Science is Raymond B. Cattell.
>
> Joseph D. Matarazzo, President of the APF, will present the Gold Medal Award for Life Achievement in Psychological Science at the 105th Annual Convention of the American Psychological Association on August 16, 1997, at 5:00 p.m. in Ballroom III of the Sheraton Chicago Hotel and Towers in Chicago. (p. 797).

The 92-year-old Cattell, with a traveling companion to assist him, traveled from his retirement home in Hawaii to be at the meeting in Chicago to receive this special honor, a gold medal award for a lifetime of work.

But Joseph D. Matarazzo did not present the Gold Medal on August 16. Instead:

> On Aug. 13, the foundation decided to postpone the



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> presentation of the award to Raymond B. Cattell, in the week proceeding the opening of the APA's 1997 Annual Convention, concerns that Cattell's writings were racist and advocated the separation of the races were voiced to the association. (http://www.apa.org/monitor/sep97/award.html).

Since its founding in 1892, the American Psychological Association (APA) has only once changed the statement of objectives contained in its bylaws. In 1892 the one objective was "to advance psychology as a science." From 1945 there have been three: "The object of the APA shall be to advance psychology as a science, as a profession, and as a means of promoting human welfare". From 1945, political concerns, left wing, became a more prominent, and contentious, part of the APA. In the files of the historian of the Psychonomic Society are letters from prominent psychologists of the time: "All manner of interests, mostly non-scientific, sprang up"; the APA proliferated into a "chaotic monster" that "fails to discriminate between science and charlatanry". It engaged in much political lobbying for mostly liberal causes. In protest, breakaway scientists formed the Psychonomic Society in 1959. Many members of the APA drifted away over the years, often in protest of the politicization of the Association. Finally a major schism occurred. In 1987/88 psychologists who wished to separate from the increasingly professional and political APA formed the American Psychological Society. Perhaps it should not be a surprise that the remnant APA was such an easy mark for the Inquisition in 1997.

The *New York Times* for August 15 reported an interview with Rhea Farberman, director of communications for the association: "Ms. Farberman said a committee had voted to give Cattell the award 'before it knew of the information that has since come to light,' adding 'This new information has raised a lot of concerns, and we want to be thorough in making a judgment'" (Hilts, 1997). This excuse of new information "coming to light" is preposterous. Cattell has never been retiring about his interpretations of data and theory. Frankly outspoken throughout his long career, his views have been widely known for decades among the scientific community. Ms. Farberman appears to be impugning the competence of the leading psychologists that had in full knowledge chosen Cattell for their most



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prestigious award. It was not even for "new information" that Cattell is on the hit list of the Inquisition; that information has been public knowledge for years.

Poor Ms. Farberman, and the APF, should have realized that with the (as yet) uncensored Internet it is becoming almost impossible to hide the most embarrassing details of organizational snafus. From winnowing great masses of Internet traffic (and admittedly some of it second-hand or further removed, and impossible to cite confidential sources) it seems that it was not new information but failure of courage that tripped up the APF. Apparently the original and lengthly letter of nomination spelled out both Cattell's scientific strengths and specifically flagged those of his views that are deemed controversial. A committee of some six well-informed past-presidents chose Cattell as deserving the Gold Medal with full knowledge of his works. Then after the award was publicly announced, a well-experienced Inquisitor, Barry Mehler (not himself a psychologist), is reputed to have threatened to disrupt the convention if the award were given to Cattell. Shades of a '60s convention in Chicago! Against much advice, and with at least one eminent psychologist threatening to resign if he did so, Matarazzo decided to cancel the ceremony and further investigate the award.

The official citation that accompanied the Gold Medal Award is as follows:

> In a remarkable 70-year career, Raymond B. Cattell has made prodigious, landmark contributions to psychology, including factor analytic mapping of the domains of personality, motivation, and abilities; exploration of three different medias of assessment; separation of fluid and crystallized intelligence; and numerous methodological innovations. Thus, Cattell became recognized in numerous substantive areas, providing a model of the complete psychologist in an age of specialization. It may be said that Cattell stands without peer in his creation of a unified theory of individual differences integrating intellectual, temperamental, and dynamic domains of personality in the context of environmental and hereditary influences. (*Amer. Psychol*, 1997, 797).

The fact is that it was Cattell's massive contribution to science



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that led to the APF decision to select him for this prestigious award, but the decision to withhold it was made on purely political grounds, i.e. that he "advocated the separation of the races."

It is that substantive and theoretical domain specified in the last two words of his citation, "hereditary influences", that long ago flagged Cattell as a target of the Inquisition. In craven response to the attack on Cattell, the APA announced that the American Psychological Foundation would now appoint a special Blue Ribbon Panel, to consist of both psychologists and nonpsychologists, to review the award.

**The Accusers**

Only two accusers have been publicly mentioned as attacking the award of the Gold Medal to Cattell. Apparently it doesn't take much to derail an organization as sensitive to the Inquisitional furies as is the APA. Neither were psychologists. The heavyweight was Abraham Foxman, identified in the *New York Times* as "the national director of the Anti-Defamation League of B'nai B'rith," who has "written to the association protesting the award, saying it would give the group's 'seal of approval to a man who has, whatever his other achievements, exhibited a lifelong commitment to racial supremacy theories.'" (Hilts, 1997).

Although it was probably the criticism of the influential ADL organization that caused the APA to hold up the award at the last moment, the initiative would seem to have come from the lesser accuser – one Barry Mehler, an associate professor of humanities at Ferris State University in Big Rapids, Michigan. Mehler has incorporated something that he calls "The Institute for the Study of Academic Racism (ISAR)".

On the Internet Mehler has provided quotes of himself: "'ISAR created this story and it's far from over,' Mehler said. 'It is gratifying to see my Institute attain this level of credibility in so short a time. I will be monitoring the investigation of the blue-ribbon committee.' ... Mehler ... has made national headlines with his recent criticism of the American Psychological Foundation's (APF) choice of psychologist Raymond B. Cattell for a lifetime achievement award .... Mehler's protest has stirred up national publicity in the *New York Times*, *The Chronicle of Higher Education*, *Reuters*, and the *Associated Press*.



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Mehler has been interviewed by radio giant WBAI in New York and has received numerous inquiries into the Cattell issue".

Mehler has also posted a sample of his writing, a paper entitled "In Genes We Trust: When Science Bows to Racism". Mehler reports that the paper was a cover feature in the magazine *Reform Judaism* for Winter 1994, and was revised and republished in four further outlets, 1, *The Public Eye*, 2, *RaceFile*, 3, *Networking: A Publication of the Fight the Right Network*, and 4, *B'nai B'rith Messenger*.

The paper is replete with passages such as "With its legacy of Dr. Josef Mengele's twin experiments at Auschwitz and Dr. Burt's bogus science, twin studies fell into disfavor". Adjectives scattered throughout include "racist", "Hitler's race ideology", "Nazi produced", "Fascist ideologist", "notoriously anti-Semitic", "fraudulent", and it concludes, "we must beware of scientists who wish to play G–d".

Such loose use of similies is reprehensible. Mehler is seemingly confusing anti-liberalism with anti-Semitism. Anti-liberalism apparently is often confused with anti-Semitism. To illustrate, in the newsletter *Details* for July 1997, published by The Jewish Policy Center, Rabbi Daniel Lapin wrote "I would like to argue that the root cause of both anti-Semitism and intermarriage in America today is the same, namely, the Jewish community's disproportionate liberalism .... The vast majority of Americans care deeply about the value of family and religion. They recognize that these institutions have been the pillars of moral society for millennia. They realize that liberalism, which devalues these institutions, is largely responsible for the fact that life in America has become more squalid, more expensive, and more dangerous over the past 30 years .... Though virtually all Americans are too decent to let this blossom into full-fledged anti-Semitism, there is always that threat." (pp. 1-2).

Mehler has been guilty of this confusion for a long time. In the book *Race, Intelligence and Bias in Academe* (Pearson, 1997), author Roger Pearson provides a chapter entitled "Activist Lysenkoism: The Case of Barry Mehler". In this he points out that decades ago Mehler was a student in a "Program for Training in Research on Institutional Racism" that was headed by Jerry Hirsch, and that Hirsch had long ago attacked Cattell. For example, he quotes Hirsch as saying "'my University of Illinois squandered a career-long research professorship



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on [Cattell].' Likening Cattell to the 'disgraced Vice President Spiro Agnew,' Hirsch railed against Cattell's 'Hitler-like recommendations' on the need for eugenic foresight" (p. 259).

Pearson continues:

> Today Hirsch is retired, and we hear less from him. But his torch is being carried by someone who appears to be even more of a zealot. That someone is his erstwhile student, Barry Mehler.
>
> Let us look at this disciple of Jerry Hirsch, an excellent example of a political activist operating from the security of the academic world. Mehler has published little or no non-political material: he appears to specialize in politicized diatribes, filled with inaccuracies, for fringe publications on the Far Left, and glories in participating in non-academic TV shows such as Geraldo. His published works have targeted respected scholars with impressive research credentials who reject the aberrant theory that all individuals and peoples are equal (i.e., identical) in their inherited potential abilities. Moreover, copies of these error-filled and scandalous attacks on such scholars have often been mailed to journalists in anonymous envelopes. Recipients have ranged from well-known figures such as Jack Anderson, the syndicated columnist, to editors of student journals and to journalists working for local newspapers in towns where the scholars Mehler lambasts work and reside. (pp. 259-260).
>
> Several qualities consistently characterize Mehler's attacks on the scholars he selects for 'exposure.' He seldom attempts to present scientific evidence to contradict the findings of their research. Clearly, since they are writing within the limits of their own or related disciplines, and he has no demonstrated or academically recognized competence in these areas, he cannot do this. Instead he falls back on *ad hominem* attacks, labeling some of America's and Britain's finest scientists 'racists,' 'nazis' and 'fascists.' Those whom he has attacked include a long list of distinguished scholars, such as: John Baker (Oxford), Thomas Bouchard (Minnesota), Sir Cyril Burt (London), Raymond Cattell (Illinois and Hawaii), C. D. Darlington (Oxford), Hans Eysenck (London), Linda Gottfredson (Deleware), James Gregor (UC Berkeley), Richard Herrnstein (Harvard), Arthur Jensen (UC Berkeley), Travis



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> Osborne (Georgia), J. Philippe Rushton (Western Ontario), Nancy Segal (Minnesota), William Shockley (Stanford), Audrey Shuey (Randolph Macon Woman's College), Ernest Van den Haag (CUNY), and Daniel Vining (Pennsylvania). (p. 262).

**The Charges**

The charges lodged against Cattell have been described at some length. There is absolutely no need here to go into any detail with regard to any of Cattell's many technical scientific achievements. This is because, true to the form described above by Pearson, the scientific accomplishments of the great man do not figure in the charges leveled against him. The charges fall into three categories: [A] heresy; [B] blasphemy; and [C] cavorting with devils. In taking the charges up one-at-a-time, I hope to show that after cutting through the invective, and discarding the gratuitous *ad hominems*, there are indeed large kernels of truth embedded in each of them. As with most victims of Inquisitions, the target is largely guilty as charged.

*[A] Heresy*. The charge is made that Cattell has been, since the 1930s, an advocate of eugenics. Indeed beyond that, Cattell followed Galton's lead in suggesting that the science of eugenics could form the basis for a new approach to religion. Cattell proposed an ethical system founded in science, to be called "Beyondism". Mehler tells us:

> Cattell first outlined his 'evolutionary ethic' based on natural selection in *Psychology and Social Progress* (1933), and that "Cattell's first monograph on the topic was, *A New Morality from Science: Beyondism* (Cattell, 1972), followed by *Beyondism: Religion from Science* (Cattell, 1987).

The invective is contemptible. Mehler tells us that "'Beyondism' is a neo-fascist contrivance. Cattell promulgates ideas that he formulated within a demimonde of radical eugenicists and neo-fascists ... it is striking for its extremism, racism, and virulent bias".

Of course the underlying heresy here, a belief in the well-established truth of genetic influence on individual differences, is totally at odds with the radical egalitarianism that is the



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Inquisition's most sacred dogma. Only with genetic causes would most of the practices advocated as eugenics be effective. People who have studied the life and works of Sir Francis Galton know that his original "eugenics" has since divided into two parts. One part, the basic science, has developed into what is today known as genetics and human genetics. The second part, the application of hereditary knowledge for the good of man and society, has developed into the largely voluntary genetic counseling of today (Whitney, 1990). Even Cattell is quoted as saying that his ideas have evolved and he is today an advocate of voluntary eugenics.

Contrary to Mehler's attempts to invoke wrath at the alleged anti-Semitism inherent in research into heredity, he should recognise, as so many Jewish scientists do, that the Jewish community has benefited from hereditarian research and eugenical practices at least as much as any other population. The case of population screening for carriers of Tay Sachs disease, followed by amniocentesis for heterozygous couples and voluntary abortion of affected fetuses, has been hailed as a great "life-giving". Parents can now choose to have a healthy baby instead of suffering through the agonizing death of a Tay Sachs affected child. For many years screening for Tay Sachs was limited to members of the Ashkenazim because they are the only population group with a relatively high frequency of the gene for Tay Sachs disease (Kaback, 1977). This is applied genetics – eugenics – in action. So too is the recently announced screening for the first identified gene that is causally linked to colorectal cancer. The screening is to be limited to Ashkenazim, the only group yet found to harbor the gene (Hopkins, 1997a, 1997b; Laken, et. al., 1997). Again, eugenics in action. It is hard to understand how such hereditarian research and application – eugenics – is in any way "anti-Semitic", as Mehler has claimed.

Other sources list many advocates in making the point that back into the 1930s and before, many social progressives of both the right and the left were enthusiastic eugenicists (Pearson, 1996). One only has to think of H. G. Wells, J. B. S. Haldane, Sidney and Beatrice Webb, and Herman J. Muller. It has also been emphasized elsewhere that the painting of eugenics with the tar brush of a slippery slope to Nazism is post-war propaganda that is largely devoid of substance (Whitney, 1996). The very recent "exposés" in the newspapers of



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governmental sponsored eugenic programs in various social democratic countries even into the 1970s (e.g. Canada, Sweden, Finland, Denmark, Switzerland) serves to underscore the lack of relationship.

The charge published in *The New York Times* that Cattell is "a man who has, whatever his other achievements, exhibited a lifelong commitment to racial supremacy theories" (Hilts, 1997) needs translation out of political invectese. Yes, it is true that as an outstanding scientist with many other achievements, Cattell has exhibited a lifelong commitment to attempting to understand the causes of both individual and group differences. Cattell is guilty of being a scientist with an interest in the causes of individual differences. As such he has followed the empirical data wherever it may have lead. As just one example of suspected environmental effects, cognitive scientists have in recent decades been very interested in the so-called Flynn effect. The finding that in industrialized societies there seems to be taking place a substantial and prolonged increase in the level of intellectual functioning (Flynn, 1987). This is a phenomenon that Cattell empirically found and reported decades ago (Cattell, 1951).

Cattell is only guilty of advocating a version of secular humanism incorporating aspects of morality and ethics that would be informed by knowledge from modern science. He named it "Beyondism".

*[B] Blasphemy.* Mehler plays the Hitler card in order to underscore Cattell's reprehensibleness:

> Hitler actually shared many values of the average American. He aimed at full employment, family values, and raising the standard of living, and countless other things, including the Volkswagen, which he designed himself for the average family. (*The Beyondist*, 1994, p. 2).

This is simply an attempt to smear Cattell by making him out to be a fan. Mehler is essentially quoting out of context. He omits the next and concluding sentence of Cattell's passage, which was:

> The man turned out evil in his militarism and his treatment



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of the Jews and dissident Catholics, but that does not justify, to a rational person, calling all his attitudes mistaken.

If we were to respond flippantly to Mehler's nonsense, we could point out that according to customer information at the Volkswagen Company (phone 1-800-822-8987), through March of 1996, 21,276,932 persons have voted with their purchase in agreement with Cattell that in the Volkswagen Hitler did a good thing.

But on a much more serious note, here it is only Mehler that is guilty of blaspheme. Mehler in effect trivializes the Holocaust by his loose and inappropriate invocation of Hitler.

*[C] Cavorting with Devils.* Mehler freely engages in guilt-by-association. Quite a few, mostly obscure or effectively marginalized, persons are named.

Wilmot Robertson seems to be the worst of the lot. Mehler says "To my knowledge, Cattell is the only major academic willing to be forthright about his association with Robertson." We are informed that Wilmot Robertson has written a few books, including one entitled *The Dispossessed Majority*, and that he publishes a "neo-fascist magazine" that is targeted toward an educated audience that is named *Instauration*. But that may be as it may be. Cattell is certainly not responsible for anything Robertson may write or publish. Neither is any interest he may or may not have in reading Robertson's publications a justifable reason for denying him a well-earned award for his contributions to science. The recent behavior of the APA seems to indicate that science is still subject to politics under the current rule of the Fourth Inquisition.

Mehler even attacks Cattell's association with *The Mankind Quarterly*, protesting that: "Cattell has published numerous times in Pearson's *Mankind Quarterly* and Pearson has published a number of Cattell's monographs." Cattell has served on the editorial board of *The Mankind Quarterly* for many years. Although that journal does not always bend to comply with the dogma of modern politicized liberalism, there is nothing even remotely anti-Semitic about its contents. Mehler may understandably resent Pearson's exposé of his



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own writings (see Pearson, 1991).[1] but Cattell's willingness to lend his name to the advisory board of *The Mankind Quarterly* in no way impugns Cattell's own status as a scholar, reflecting only to the credit of *The Mankind Quarterly*.

**Another View**

An eminent student of the human condition, the recently deceased Hans J. Eysenck, once addressed the very issues that now face the Blue Ribbon Panel convened by the APF to look into Cattell's Gold Medal:

> This, then, is the "trahison des clercs" of which I make complaint: that both students and their elders and betters have begun to play a child's game of goodies and baddies, in which a man's work is judged, not in terms of its scientific content, or on any rational, empirical basis, but in terms of whether it agrees with the critic's preconceptions.
>
> And my suggestion for the future is that which Sir Francis Bacon gave centuries ago in *The Advancement of Learning*:
>
> > "To have the true testimonies of learning to be better heard, without the interruption of tacit objection, I think good to deliver it from the discredits and disgraces it hath received, all from ignorance; but ignorance severally disguised; appearing sometimes in the zeal and jealousy of devines; sometimes in the severity and arrogance of politiques; and sometimes in the errors and imperfections of learned men themselves ..."
>
> However that may be, there are of course difficult ethical and moral problems and dilemmas involved in the discussion, and the exhortations of militant Leftists should not preclude serious

[1] In this work Pearson tells us that Mehler has published in "the Marxist-aligned *Science for the People*" (p.263), and "in *The Guardian News Weekly*, which described itself as a 'Marxist-Leninist radical newspaper'" (p. 265). On the other hand, Mehler refers to "Fascist ideologist Roger Pearson ..." and seems displeased that Pearson served on the original board of editors for the Heritage Foundation's highly respected *Policy Review*. The final outrage seems to be that President Ronald Reagan personally sent Roger Pearson a letter of commendation.



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discussion of these problems. Note first of all a 'Resolution in Scientific Freedom,' signed by 50 eminent scientists, among them: Francis H.C. Crick, Nobel Prize-winner, Cambridge University; Jacques Monod, Nobel Prize-winner, College de France; Arthur R. Jensen, University of California; Richard Herrnstein, Harvard University; C.D. Darlington, Oxford University; and John C. Kendrew, Nobel Prize-winner, Cambridge University. The Resolution reads as follows:

> "The history of civilization shows many periods when scientific research or teaching was censured, punished, or suppressed for non-scientific reasons, usually for seeming to contradict some religious or political belief. Well-known scientist victims include: Galileo in orthodox Italy; Darwin, in Victorian England; Einstein, in Hitler's Germany; and Mendelian biologists, in Stalin's Russia."
>
> "Today, a similar suppression, censure, punishment, and defamation are being applied against scientists who emphasize the role of heredity in human behavior. Published positions are often misquoted and misrepresented; emotional appeals replace scientific reasoning; arguments are directed against the man rather than against the evidence (e.g. a scientist is called 'fascist', and his arguments are ignored).
>
> "A large number of attacks come from non-scientists, or even anti-scientists, among the political militants on campus. Other attackers include academics committed to environmentalism in their explanation of almost all human differences. And a large number of scientists, who have studied the evidence and are persuaded of the great role played by heredity in human behavior, are silent, neither expressing their beliefs clearly in public, nor rallying strongly to the defence of their more outspoken colleagues.
>
> "The results are seen in the present academy; it is virtually heresy to express a hereditarian view, or to recommend further study of the biological bases of behavior. A kind of orthodox environmentalism dominates the liberal academy, and strongly inhibits teachers, researchers, and scholars from turning to biological explanations or efforts. Now, therefore, we the undersigned scientists from a variety of fields, declare the



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following beliefs and principles:

"(1) We have investigated much evidence concerning the possible role of inheritance in human abilities and behaviors, and "we believe such hereditary influences" are very strong.

"(2) We wish strongly to encourage research into the biological and hereditary bases of behavior, as a major complement to the environmental efforts at explanation.

"(3) We strongly defend the right, and emphasize the scholarly duty, of the teacher to discuss hereditary influences on behavior, in appropriate settings and with responsible scholarship.

"(4) We deplore the evasion of hereditary reasoning in current textbooks, and the failure to give responsible weight to heredity in disciplines such as sociology, social psychology, social anthropology, educational psychology, psychological measurement, and many others.

"(5) We call upon liberal academics - upon faculty senates, upon professional and learned societies, upon the American Association of University Professors, upon the American Civil Liberties Union, upon the University Centres for Rational Alternatives, upon presidents and boards of trustees, upon departments of science, and upon the editors of scholarly journals - to insist upon the openness of social science to the well-grounded claims of the bio-behavioral reasoning, and to protect vigilantly any qualified faculty members who responsibly teach, research, or publish concerning such reasoning.

"We so urge because as scientists we believe that human problems may best be remedied by increased human knowledge, and that such increases in knowledge lead much more probably to the enhancement of human happiness, than to the opposite."

I was asked by the British Association for the Advancement of Science to contribute an article on the ethics of science and the duties of scientists, with special reference to these events. What I wrote then I still believe to be right, and consequently the body of the text of my contribution is reprinted here in full. This is what I said:

"It used to be taken for granted that it was not only ethically



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'right' for scientists to make public their discoveries; it was regarded as their 'duty' to do so. Secrecy, the withholding of information, and the refusal to communicate knowledge were rightly regarded as cardinal sins against the scientific ethos. This is true no more. In recent years it has been argued, more and more vociferously, that scientists should have regard for the social consequences of their discoveries, and of their pronouncements; if these consequences are undesirable, the research in the area involved should be terminated, and results already achieved should not be publicized. The area which has seen most of this kind of argumentation is of course that concerned with the inheritance of intelligence, and with racial differences in ability; many even of those who acknowledge that Jensen's arguments are scientifically correct have argued that he was wrong (and that Herrnstein and I were wrong) in actually publishing the conclusions to which all the experimental work was leading. Stressing the possible hereditary nature of the IQ deficit of American blacks, as compared with American whites, might have serious consequences in jeopardizing the integration between the races so earnestly desired by both sides to the argument; carrying out further research might offend liberal opinion, and lead to further dispute, strife, and even bloodshed. What good could come of work along these lines, it was frequently argued; the results would be of purely academic interest as both sides were agreed that there was much overlap in ability between the two races, so that each individual would still have to be judged in terms of his particular pattern of abilities, rather than as a member of a particular race. Better let sleeping dogs lie and studiously turn a blind eye to such facts and theories as might impinge on the general belief in universal egalitarianism, and threaten its very foundations.

"I believe that there are powerful arguments against this modern belief in the opportunistic silencing of inconvenient theories, and the refusal to support research which might unearth equally inconvenient facts, all in the supposed interests of society. The first argument by itself, I would suggest, is quite conclusive; it is based on the impossibility of forecasting the social consequences (or even the scientific consequences) of one's findings and theories. It is impossible to read the history



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of science without becoming aware of the fact that even the greatest scientists were incapable of looking ahead even a few years and predicting the consequences of their actions." (Eysenck, 1997, pp 45-48).

## Guilty as Charged

With regard to Giordano Bruno, "His reward indeed came – even for his faulty utterances – when, toward the end of the nineteenth century, thoughtful men from all parts of the world united in erecting his statue on the spot where he had been burned by the Roman Inquisition nearly three hundred years before." (White, 1896/1965, p.80).

We can only hope that the Blue Ribbon Panel of the APF can render its verdict with regard to Raymond B. Cattell in a more timely fashion.

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# Intelligence and National Achievement

*Editor*

***Raymond B. Cattell***

**CONTENTS INCLUDE**


INTRODUCTION: THE CONTEMPORARY AWAKENING
Raymond B. Cattell

THE ROLE OF PSYCHOLOGICAL TESTING
Raymond B. Cattell

TEST SCORES AS MEASURES OF HUMAN CAPITAL
Barbara Lerner

FERTILITY DIFFERENTIALS AND THE STATUS OF NATIONS:
A SPECULATIVE ESSAY ON JAPAN AND THE WEST
Daniel R. Vining, Jr.

POPULATION INTELLIGENCE AND NATIONAL SYNTALITY
Raymond B. Cattell and Jerry M. Brennan

APPENDIX: SOME CHANGES IN SOCIAL LIFE IN A COMMUNITY
WITH A FALLING INTELLIGENCE QUOTIENT
Raymond B. Cattell


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# Raymond Bernard Cattell: A Lifetime of Achievement

Kevin Lamb
*Washington D.C.*

World-renowned for his academic integrity, industriousness and commitment to scientific objectivity, *Mankind Quarterly* editorial board member Raymond B. Cattell has made massive contributions to behavioral science which have altered the direction and pace of research in various psychological fields. These include personality, intelligence, temperament, human motivation, learning and multivariate behavioral analysis. However, his concerns about the future of mankind's genetic heritage and about the need to apply scientific knowledge to the resolution of ethical dilemmas have led a small group of non-scientists to exert pressure to suppress professional recognition of his exceptional contributions to psychological science on grounds that had nothing to do with the magnitude of his contribution to science.

KEY WORDS: Raymond B. Cattell, American Psychological Foundation, Multivariate Behavioral Research, IQ, Personality.

In historic terms, the achievements of creative intellects such as Galileo, Newton and Darwin have generated a fervent degree of criticism. Over the years the salience of Raymond Bernard Cattell's impressive record of accomplishments has likewise fueled criticism from hostile social ideologues. Despite these shallow critiques, no one anticipated the intense eleventh-hour pressure that forced the American Psychological Foundation to withhold their lifetime achievement award to one of the most qualified candidates ever to be selected for this recognition. The following biographical sketch borrows heavily from the material prepared by Professor John L. Horn for submission to the American Psychological Foundation as a part of the nominating process which led the APF awards committee, comprising six former presidents, to grant professor Cattell the 1997 Gold Medal Award for Lifetime Achievement in Psychological Science. The decision to withhold the award after it had been publicly announced and after Cattell had travelled from Honolulu to Chicago



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to receive it, was made as a result of pressure from a small group of non-psychologists on grounds that were purely political.

**Early Life**

Born in England in 1905, Raymond Bernard Cattell was the second of three sons raised in a gifted family with solid English and Scottish roots. His family moved from Staffordshire (in England) to Devonshire when he was six years of age, and he therefore spent the formative years of his childhood in the Devonshire countryside.

Following the tradition of his father and grandfather's family-run manufacturing and engineering factories, Cattell's two brothers took up engineering as a profession. Raymond Cattell, however, chose differently. In 1924 he won a scholarship to King's College, Cambridge University, at the exceptionally early age of fifteen and just four years later, at the age of nineteen, he graduated with first class honors with a Bachelor of Science degree in Chemistry. He was obviously a child of exceptional intelligence, his father's IQ having been recorded as 120, and his mother's as 150.

During this period, Cattell's scientific interests shifted from chemistry to the emerging field of psychology. This decision was made after attending a lecture by the eminent British psychologist, Sir Cyril Burt. Cattell's first publication ("The Significance of the Actual Resistance in Psycho-Galvanic Experiments") appeared in the *British Journal of Psychology* in 1928, while he was still engaged in postgraduate studies at the University of London under the direction of Charles Spearman. He won his Ph.D. in 1928, and his dissertation on *The Subjective Character of Cognition* was published as a *British Journal of Psychology* monograph in 1931.

Early in his career, Cattell developed two fundamental interests that have influenced the scope and direction of his life's work: the importance of establishing an objective framework from which to study individual differences in human behavior and the necessity of applying scientific principles to social, political and economic problems. These two priorities reflected common threads which ran throughout much of his research.

**Academic Career**

Cattell began his professional career as a Lecturer in Psychology



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at the University College of the Southwest at Exeter, which then prepared students for University of London degrees (it is today the University of Exeter). While teaching there, in 1930 he developed the Cattell Intelligence Tests, Scales 1, 2 and 3, which were administered to individuals ranging from four years of age to adulthood. Another early book appeared the next year, an English translation of Ernst Kretschmer's *The Psychology of Men of Genius*.

Cattell received a Child Guidance Council Fellowship in Clinical Psychology in 1932 and began to write on the description and measurement of personality. His 1933 book, *Psychology and Social Progress*, revealed his insightful scientific analysis of social problems. Several popular beliefs and conventional ideas were critically examined therein, including the view that Cattell articulated over the course of his professional career, namely that the solution to social, economic and moral problems ultimately rests with the development of rigorous, objective psychological research on which to base moral decisions. His favorable attitude towards eugenics dates from this period. One year later, in 1934, he wrote *Your Mind and Mine*, a small popular textbook explaining psychology that influenced the style of writing introductory psychology textbooks.

Between 1934 and 1937, Cattell served as Director of the City of Leicester's Child Guidance Clinic. During this period, he produced a number of papers on character development, the measurement of perseveration, the theory of motivation and the nature of intelligence. *A Guide to Mental Testing*, which went through three different editions, appeared in 1936.

Cattell's first book of major importance, *The Fight for our National Intelligence*, was published in 1937. This expounded his theories on the inheritance of mental and other abilities, and his fears concerning the decline of intelligence in Britain due to disparate birth rates between those of high and those of low intelligence. With the aid of a fellowship grant, named in honor of Charles Darwin's son Major Leonard Darwin, a former presiding officer of the (British) Eugenics Society, Cattell studied inter-generational shifts in intelligence levels in the small villages of the Dartmoor area and concluded that such a shift represented a growing dysgenic decline in native intelligence. As revealed in the following passages, Cattell's analysis was blunt, bold and direct:



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> Of all the complacent, unscientific thinking of our age the worst is that which treats the slum problem as if it were a question of bricks and mortar. Dr. J. J. Mallon writes, in support of further subsidized building, and admitting that "the government has not been inactive or the acts ungenerous" ... Unlike the mills of god they do not grind to the tune of the parable of the talents, but to a crazy modern song of indulgence that has disaster at the end of it. It has taken a bishop to remind us that "the slumite is a far worse problem than the slum," but the fact is deducible easily enough from the fact that hundreds of well-built spacious streets in London and other cities are now slums.
>
> The educator used to speak of training intelligence: he now speaks more modestly of teaching people to use in reasoning what intelligence they possess; and even this kind of training is a two-edged weapon. You can train an imbecile or an ape to use a knife or a fork, but you cannot train him to decide when to use either.
>
> To the people who want the benefits of civilization without its restraints, the community will have to say, "No public assistance without control of birthrates." Only by thus facing up to the principle that if the state is to feed, educate and support children, it has a right to control their production, will the community put an end to the grip of this social blackmailer.
>
> Until the horse of demand and the cart of reproduction rate are harnessed together again culture can perform a eugenic service only by acting as an indicator of the retreat of capacity. Not all aspects of culture can do this: no one will notice when the percentage of people able to appreciate Shakespeare falls off, but when arts and industry decay, then (as is now beginning) there comes a shortage of skilled men in engineering, even the dullard will begin to cry havoc. (p. 93.)

This concern with the genetic quality of posterity was something that was to haunt Cattell right up to the present moment of time. As John B. Carroll wrote in *Multivariate Behavioral Research* (p. 307, April/July 1994):

> In that volume he argued that in Britain those of low intelligence were having more children, at an earlier age, than were people of high intelligence, with the result that the average



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> capability of the population was declining generation by generation. Continued over several generations this trend would produce a number of quite undesirable outcomes, he said; something should be done to reverse this trend. The call was for one kind of people to breed less, another kind to breed more.
>
> These arguments appeared in a social context in which *Mein Kampf* and Hitler's rise to power were everyday topics of conversation. In such times Cattell's book was often cited and "mis-cited" ... There is little doubt that Cattell's association with eugenics and genetical research discouraged study of the Cattellian system.

The issue of a dysgenic decline in IQ levels is one that continues to surface in behavioral genetic research. The most recent examples of which include a discussion of dysgenic population trends in Herrnstein and Murray's *The Bell Curve*, Richard Lynn's review of the evidence to date in his 1996 book *Dysgenics: Genetic Deterioration in Modern Populations* and, even more recently, as a focal point of analysis on IQ and social trends in the pages of the the November 1997 issue of the *American Psychologist*. Even though Cattell has been accused of misjudging the magnitude of such a dysgenic decline, any reasonable assessment would conclude that Cattell was able to see both the forest and the trees.

Two other books by Cattell appeared in 1937: *Human Affairs*, a collection of essays (co-edited with two other social scientists) that included papers by J.B.S. Haldane, Havelock Ellis and William McDougall; and *Under Sail Through Red Devon*, an account of Cattell's sailing adventures off the coast of Devon – for he was an academic who enjoyed the outdoors. That year, he decided to move to the United States to work with Edward Lee Thorndike at Columbia University.

*Psychology and the Religious Quest*, published the next year, put religion into its psychological niche, showing how ethics was rooted in human psychology, and advancing practical recommendations aimed at bringing society into line with pyschological needs. One year later he was appointed G. Stanley Hall Professor of Genetic Psychology at Clark University, and in 1939 he published his Culture-Free Intelligence Test, and was awarded a D.Sc. from the University



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of London.

In 1941, Cattell published his *General Psychology* in a series edited by Louis Terman. This basic textbook introduced the concept of multiple factor analysis. Some complained that this innovation was too sophisticated in content and was above the ability level of introductory students, but Cattell maintained that the introduction of this concept at this level was necessary to the understanding of problems which could not be handled without it. As Cattell explained:

> It is the teacher's duty to consider the interests and abilities of the student, but it is a duty also to lead the student to appreciate values in fact and reasoning.
>
> In the end the student has to appreciate psychology as a science, as rigid as any other science, and this is not achieved by giving him a false sense of understanding through smoothing over difficulties.

Also in 1941, Cattell was appointed a Fellow in Psychology at Harvard University, and during the next few years he advanced views on the nature of human intelligence that were eventually submitted to empirical scrutiny with the assistance of his graduate student, John L. Horn. Cattell's triadic theory of intelligence was based on two core factors: fluid (Gf) which developed at an earlier age and declined with increasing age, and crystallized (Gc) intelligence which tended to increase with age. In 1944 he published a paper entitled "The Place of Religion and Ethics in a Civilization Based on Science," evaluating the contemporary society in psychological terms.

During 1944-45 Cattell briefly aided the Personnel Research Division of the Adjutant General's Office in developing personality tests, then, in 1945, he accepted a position as Research Professor in Psychology at the University of Illinois, Champaign-Urbana, where he spent nearly three decades refining both his theoretical and empirical studies of intelligence and personality.

Between 1946-1950, Cattell published three major textbooks on personality, which have been deemed by some psychologists to be amongst the most influential books of this century. These were *The Description and Measurement of Personality* (1946), *An Introduction to Personality Study* (1949), and the even more important *Personality, a Systematic, Theoretical, and Factual Study* (1950). As John L. Horn



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reported in his submission to the American Psychological Foundation, these books:

> .... developed a methodologically sophisticated and theoretically comprehensive program of research by systematically studying the phenomena of personality through measures of self-reports (questionnaires), ratings (the perceptions of others) and objective test performances. This plan of attack influenced the entire field of psychology. The theory Cattell developed came to be characterized as trait theory – in particular, multivariate trait theory – but in fact it took into account, also, evanescent states and other changing structures.

Over the course of his work at the University of Illinois, Cattell published a number of books on personality, factor analysis, intelligence as well as neuroticism and anxiety. His 1952 *Factor Analysis* became an example for those writing textbooks in this subject, and in 1957 he wrote *Personality and Motivation Structure and Measurement*, another landmark text that summarized the empirical research produced through the 1940s and 1950s. Similarly his 1962 *Meaning and Measurement of Neuroticism and Anxiety*, advancing an empirically-based theory of state and trait anxiety, exerted a major influence on the thinking of other psychologists in this field. As the August 1997 *American Psychologist*, announcing the award of the APF Gold Medal (subsequently withheld), states that "Cattell's work on anxiety and neuroticism has become the core of modern theory on state and trait anxiety."

As the 1960s advanced, Cattell devoted increasing time to research into the nature and structure of ability, and in conjunction with his research Cattell established the Society of Multivariate Experimental Research in 1960, and in 1963 Cattell and Horn jointly established the empirical nature of *Gf* and *Gc*. His *Handbook of the Sixteen Personality Factor Questionnaire* was published in 1970 and his single most significant work on intelligence, *Abilities: Their Structure, Growth and Action*, appeared in 1971 following his 1960s series of books on personality measurement, creativity and achievement.

One of the more original and thought-provoking volumes of Cattell's texts, *A New Morality From Science: Beyondism*, was released



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in 1972. In this he argued that Man's prime moral responsibility was to posterity, and because he perceived that Man's inherent nature was genetically determined, eugenics ought to play a major role in the development of any and all social programs. Unfortunately the idea that religion should be rooted in reality was too much for most academics and non-academics, as it seemingly still is today, and T. Haag expressed the general reaction to this book when he wrote:

> **It is a thinking man's book, and of such stuff best sellers are rarely made. But the power to provoke thought is the first trait of a classic; perhaps that is the fate marked out for this work.**

Cattell retired from the University of Illinois in 1973, but over the course of his retirement years he has produced sixteen monographs and contributed papers to several edited works in the study of personality measurement and intelligence testing. During this period he also published an integrated theory of human learning that synthesized the input of environmental and cultural variables. This was his two volume *Personality and Learning Theory* (1979/80). Two years later, in 1982, he further published the behavioral genetic equivalent of a scientific roadmap entitled *The Inheritance of Personality and Ability: Research methods and findings* (1982).

In conjunction with his research Cattell established the Society of Multivariate Experimental Research, which today awards an annual Cattell Award for Distinguished Multivariate Behavioral Research. The American Educational Research Association awards an annual Raymond B. Cattell award. Cattell is himself a recipient of the prestigious Darwin Fellowship, the Wenner-Gren Prize of the New York Academy of Science, the Psychometric Award of the APA/Educational Testing Service, the Dobzhansky Award for lifetime achievement of the Behavior Genetics Society, and the U.S. Department of Education Award for Academic Contributions. Perhaps even more significant are the numbers of Cattell's former students who have made major contributions to their own chosen fields of psychology.

The *American Psychologist* of August 1997, announcing the APF award which was subsequently withheld, stated that:



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> .... in his remarkable seventy-year career, Cattell wrote 56 books and made prodigious landmark contributions to psychology. He must be considered among a very small handful of people in this century who have most influenced the shape of psychology as a science.

For someone who wanted nothing more than to increase human knowledge and to secure the proper advancement of future generations of mankind, Raymond Bernard Cattell has been recklessly maligned by a handful of radical ideologues who dislike the pro-eugenic views which he developed in the course of his research, but who are unable to fault either his professional contributions to psychology or his widely acknowledged excellence in his chosen field of study.

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*Based upon the Bibliography prepared by John L. Horn*

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## BOOK REVIEWS

**The World Food Outlook**
*Donald O. Mitchell, Merlinda D. Ingco and Ronald C. Duncan*
Cambridge University Press, U.K.

The authors of this well-researched book seem to be totally blind to the harmful effects of uncontrolled population expansion in the Third World and to the resultant environmental degredation. Like many of of the world's contemporary economists they seem to believe that there is no value that cannot be appropriately measured in money. Thus, concludinging that modern technology is capable of expanding food production to support the continued increase in human population, they delight in declaring Malthus to have been proven wrong. Without agreeing with that contention, this reviewer would suggest that they ought to be lamenting the fact that the human population has continued to expand at hitherto unsupportable rates. The present vast increase of human beings is destroying the biosphere and driving other advanced species of plants and animals into extinction. The world is already overpopulated by *Homo sapiens* not only to the detriment of future generations, but also to the detriment of all living things other than the parasites that live off humanity.

Thus we find the authors proclaiming that:

> modern-day Malthusians warn that Malthus will ultimately be right: the world will be less and less able to feed itself. They are anxious to apply their pessimism to developing countries – population keeps expanding, no new land is being created, crop yields have increased considerably and may have peaked, and the environment may not tolerate the pressure of more intensive agriculture. We read that China's demand for grain will exceed its own production capacity and take the rest of the world's surplus – Malthus is moving east! While these arguments seem persuasive, the evidence to the contrary is compelling: prices of agricultural commodities in real terms are



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> at their lowest level in history, and crop output continues to rise faster than population.
>
> The fact is that the world food situation has improved dramatically for most of the world's consumers. Output of cereals, the world's main food source, has increased 2.7 per cent per annum since 1950, while population has grown by about 1.9 per cent per annum. Cereal yields have increased at 2.25 per cent per annum. Not all people in the world today have adequate diets and there is no doubting the desperate circumstances of some peoples, but diets for most of the world's consumers have improved dramatically and per capita calorie consumption in developing economies has increased by some 27 per cent since the 1960s. It should continue to improve, and food will be cheaper than it is today.
>
> Sub-Saharan Africa remains the primary exception to these general developments with stagnant or even declining per capita consumption levels.

According to this, Africa is the only bleak spot on the earth's surface. But the future does not look so rosy when we take into consideration the Asian Development Bank's recent statements about pollution and environmental degradation on that continent: Asia contains 13 of the 15 most polluted cities in the world (and the population of these is increasing and pollution worsening); Asian rivers are nearly four times as polluted as they were in the 1970s; one percent of Asia's forests are being lost *every year*; between 70 and 90 percent of the region's animal life has already disappeared; and over vast areas the deforested soil is being lost through erosion ("Fight pollution or sink in it, Asia told" by F. Darlan, *The Age*. Australia, May 12, 1997).

Have the ivory towers inhabited by economists and university presses no windows that permit their residents to see what is going on around them? Malthusian controls over the human species may well have been negated up to the present, but it would have been better for posterity if they had kicked in earlier – or if voluntary population management policies had been pursued in a timely and prudent fashion.



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**The Pivot of Civilization**
*Margaret Sanger*
*Introduction by H.G. Wells*
Scott-Townsend Publishers
ISBN 1-878465-27-9 304 pp. Price $20
1997 (Reprint of original 1922 edition)

This is a reprint of one of Margaret Sanger's best known pivotal works on birth control and over-population. Margaret Sanger, née Higgins, was a trained nurse who became a historic pioneer of women's rights, birth control, and something more remarkable by today's popular standards – eugenics. Born in 1883, her experiences impressed upon her the plight of poor families who were unable to care for their often unwanted and overly numerous offspring. In consequence she became a heroine of the early feminist movement – of women who claimed the right to control their own bodies and to give birth only to children they wanted to bear. However, in this present era of biological egalitarianism, Margaret Sanger has become a controversial figure.

Recognizing the suffering that poverty and over-population brought in its wake, Margaret Sanger also came to appreciate the importance of heredity, and as the years passed she became even more concerned about the quality of succeeding generations. It was those who had sunk into the ghetto who tended to produce the most offspring, she noted, and consequently it was those who had failed in life who passed on their genes in greater quantity to the next generation. Birth control, a term she invented, should be linked to eugenic policies. Touring the world to alert humankind to the importance of demographic trends, she founded and became the first president of the American Birth Control League, a name which was later changed to the Planned Parenthood Federation of America.

But Margaret Sanger, committed as she was to the suffering of poor mothers who strove to care for large numbers of unplanned children, thought much more deeply than the modern feminists – and as a result she has now come under attack as a "racist." Thus, a recent *Wall Street Journal* article ("The Repackaging of Margaret Sanger" 5/5/97) complained that in her book *The Pivot of Civilization* "she called for the extirpation of 'weeds ... overrunning the human garden';



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for the segregation of 'morons, misfits, and the maladjusted'; and for the sterilization of 'genetically inferior races'" (in all, a gross misrepresentation of her position, since she was an advocate of voluntary eugenic self-selection). The author of the article went on to write that "it was later that she singled out the Chinese, writing in her autobiography about 'the incessant fertility of [the Chinese] millions spread like a plague.'" Nobody today, or even in her day, would accuse the Chinese of being unintelligent, but those Asian countries that are close to China truly fear the migrant pressure of the 1.25 billion Chinese, with even the U.S.A. complaining about the wholesale smuggling of illegal Chinese immigrants into the U.S.

When Margaret Sanger was born, the world population of human beings stood at less than one billion; today it is five and a half times that number. Sanger believed that overpopulation promoted not merely poverty but environmental degradation, social conflict, massive disruptive international migration, and war. She foresaw the plight of Africa, Asia and Latin America, burdened by vast and continuously worsening overpopulation, and she foresaw the migratory pressures that would result from the charity of the more prosperous Western world.

The prime thrust of the current attack on Margaret Sanger is directed against her concern that it was the less competent inhabitants of the Western cities and of the Third World who were over-reproducing themselves. The leaders of Western society, she claimed, were following a policy of misguided charity which was promoting the production of "moron communities." There was a need, she felt, to reduce the world's birthrate by freeing women to make their own choices regarding child-bearing. Efforts to make birth control prophylactics available internationally, she argued, would reduce the birthrate and increase the chances that the children who entered the world would be wanted children, born of fit, healthy and competent stock. In short, Margaret Sanger, early pioneer of women's rights, was a very modern person who foresaw the plight of our contemporary world and sought to warn against it.

*The Pivot of Civilization*, originally published in 1922, was one of her best-known books (although her views were even more forcefully expressed in her subsequent autobiography). This reprint consequently enables readers to judge for themselves the validity of



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Margaret Sanger's arguments, and of the attacks levelled against her. In retrospect, her fears about world over-population – when "battalions of unwanted children" would not only destroy the earth's ecological balance but also inundate even the more successful nations in their urge to escape the conditions that their own high rate of fertility had created – today seem warranted.

Nevertheless, contrary to the impression created by her critics, Sanger actually condemned those of her own race who warned that the West was committing "race suicide" by failing to maintain its own numbers vis-a-vis the swelling population of the Third World. She herself believed that the West could prevent the impending disaster by liberating the women of Asia, Africa and Latin America from the need to undergo unwanted pregnancies – and by so doing avoid the demographic explosion. U.N. efforts at capping the world population explosion have followed this strategy, but these hopes have not been realized.

Yet neither Margaret Sanger nor the leaders of our contemporary world are as outspoken as H.G. Wells is in his Introduction to this book. This is what H.G. Wells, socialist and reformer, writes in *The Pivot of Civilization:* "[modern science] sweeps away the venerable checks of pestilence and disease, and confronts us with the congestions and explosive dangers of an over-populated world ... we cannot go on giving you health, freedom, enlargement, limitless wealth, if all our gifts to you are to be swamped by an indiscriminate torrent of progeny. We want fewer and better children ... and we cannot make the social life and the world-peace we are determined to make, with the ill-bred, ill-trained, swarms of inferior citizens you inflict upon us... I am inclined to think that there has hitherto been rather too much personal emotion spent upon this business, and far too little attention given to its broader aspects."

Margaret Sanger's epochal work makes excellent contemporary reading.



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